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CONDUCTED BY

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

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“Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbēs
Tam vario motu.”

J. B. Pinelli ad Mazonium.

CONTENTS OF VOL. XXXI.

(FOURTH SERIES.)

NUMBER CCVI.—JANUARY 1866.

	Page
Mr. T. R. Edmonds on the Law of Human Mortality expressed by a New Formula	1
Sir David Brewster on the Bands formed by the Superposition of Paragenic Spectra produced by the Grooved Surfaces of Glass and Steel.—Part I. (With a Plate.)	22
Mr. J. Croll on the Excentricity of the Earth's Orbit.	26
Prof. Clausius on the determination of the Disgregation of a Body, and on the True Capacity for Heat	28
Prof. Challis on Hydrodynamics.—Part III.	33
Mr. R. Edmonds on Earthquakes and extraordinary Agitations of the Sea.....	45
Prof. Sylvester on Astronomical Prolusions: commencing with an instantaneous proof of Lambert's and Euler's Theorems, and modulating through a construction of the orbit of a heavenly body from two heliocentric distances, the subtended chord, and the periodic time, and the focal theory of Cartesian Ovals, into a discussion of motion in a circle and its relation to planetary motion	52
Notices respecting New Books :—The Mathematical Writings of D. F. Gregory, M.A.	76
Proceedings of the Cambridge Philosophical Society :—	
Prof. Cayley on a new Theorem on the Equilibrium of four forces acting on a Solid Body	78
Prof. Sedgwick on the Geology of the Valley of Dent, with some account of a destructive Avalanche which fell in the year 1752	79
Mr. J. W. Clark on the Rib of a Whale found near Cromer	81
Mr. G. F. Browne on some Ice-caves	82
On the Density of Ozone, by M. Soret	82
Note of an Experiment on Voltaic Conduction, by J. J. Waterston.	83
On the Coloration of Glass by Selenium, by M. J. Pelouze	84

NUMBER CCVII.—FEBRUARY.

Dr. E. Rose on the Doctrine of Colour-disease	85
Sir David Brewster on the Bands formed by the Superposition of Paragenic Spectra produced by the Grooved Surfaces of Glass and Steel.—Part II. (With Two Plates.)	98

	Page
Mr. J. M. Wilson's Remarks on an observation of Mr. Glaisher's	104
M. E. Wartmann on the Explosive Distance of the direct induced Current between Electrodes of the same kind	107
Mr. J. P. Cooke on the Construction of a Spectroscope with a number of Prisms, by which the angle of minimum deviation for any ray may be accurately measured and its position in the solar spectrum determined	110
Mr. J. Gill on Regelation	119
Dr. H. W. S. van der Kolk's Studies on Gases	124
Dr. Atkinson's Chemical Notices from Foreign Journals	137
Notices respecting New Books:—Archdeacon Pratt's Treatise on Attractions, Laplace's Functions, and the Figure of the Earth	144
Proceedings of the Royal Society:—	
Dr. A. Matthiessen on the Expansion of Water and Mercury	149
Prof. W. H. Miller on the Forms of some Compounds of Thallium	153
Proceedings of the Geological Society:—	
Mr. P. M. Duncan on the impressions of Selenite in the Woolwich Beds and London Clay	155
The Rev. O. Fisher on the Relation of the Chillesford Beds to the Norwich Crag	156
Mr. E. B. Tawney on the Western Limit of the Rhætic Beds in South Wales	157
The Rev. P. B. Brodie on a Section of Lower Lias and Rhætic Beds near Wells, Somerset	157
Dr. J. W. Dawson on the Conditions of the Deposition of Coal	158
Prof. W. King and Dr. T. H. Rowney on the Origin and Microscopic Structure of the so-called Eozoon-Serpentine	159
Dr. W. B. Carpenter on the Structure and Affinities of <i>Eozoon Canadense</i>	159
On the Measurement of Small Forces by means of the Pendulum, by MM. Jamin and Briot	160
On the Expansion of Saturated Vapours, by M. A. Cazin	163

NUMBER CCVIII.—MARCH.

Prof. How's Contributions to the Mineralogy of Nova Scotia..	165
Mr. J. M. Wilson on some Problems in Chances	170
Archdeacon Pratt on the Level of the Sea during the Glacial Epoch in the Northern Hemisphere	172
Dr. E. J. Chapman on some Minerals from Lake Superior	176
Dr. H. W. S. van der Kolk's Studies on Gases	181
Prof. Tyndall on the Black-bulb Thermometer	191
Captain A. R. Clarke on Archdeacon Pratt's 'Figure of the Earth'	193

	Page
Dr. Rankine on the Expansion of Saturated Vapours	197
Dr. Rankine on Saturated Vapours	199
Mr. D. D. Heath on Secular Local Changes in the Sea-level. .	201
Prof. Guthrie's Speculation concerning the relation between the Axial Rotation of the Earth, and the Resistance, Elasticity, and Weight of Solar Æther	210
Prof. Sylvester on an improved form of Statement of the New Rule for the Separation of the Roots of an Algebraical Equa- tion, with a Postscript containing a New Theorem.	214
Prof. Challis on the Solution of a Problem in the Calculus of Variations by a New Method	218
Mr. J. Broughton on some Properties of Soap-bubbles	228
Proceedings of the Cambridge Philosophical Society:—	
Dr. Drosier on the Functions of the Air-cells, and the Me- chanism of Respiration, in Birds	230
Proceedings of the Royal Society:—	
Mr. W. Huggins on the Spectrum of Comet 1, 1866	233
Mr. B. Stewart on the Secular Change of Magnetic Dip, as recorded at the Kew Observatory.	235
Proceedings of the Geological Society:—	
Mr. R. A. C. Godwin-Austen on Belgian Geology.	237
On the Changes which Stretching and the passage of a Voltaic Current produce in a Magnetic Bar, by M. Villari of Naples. .	239
On the Heat of Friction, by Prof. Josiah P. Cooke, Jun.	241
Note regarding the decrease of Actinic Effect near the circum- ference of the Sun, as shown by the Kew Pictures, by Messrs. Warren De la Rue, Stewart, and Loewy	243

NUMBER CCIX.—APRIL.

Dr. Stevelly on the Composition of Forces	245
Dr. Heddle on the occurrence of Wulfenite in Kirkcudbrightshire	253
M. E. Edlund on the Heat disengaged by Induction-currents, and on the relation between this disengagement of Heat and the mechanical force employed to produce it	253
Mr. J. M. Wilson on the Diminution of Direct Solar Heat in the Upper Regions of the Atmosphere	261
Prof. Norton on Molecular Physics	265
Prof. J. A. Wanklyn on the Doctrine of Uniform and Constant Saturation.	283
Prof. Sylvester on the Periodical Changes of Orbit, under certain circumstances, of a particle acted on by a central force, and on Vectorial Coordinates, &c., together with a new Theory of the Analogues to the Cartesian Ovals in Space, being a Sequel to "Astronomical Prolusions"	287
Mr. J. Croll on the Physical Cause of the Submergence and Emergence of the Land during the Glacial Epoch. With a Note by Professor W. Thomson, F.R.S.	301
Dr. Atkinson's Chemical Notices from Foreign Journals.	306

	Page
Proceedings of the Cambridge Philosophical Society :—	
Prof. C. Babington on the Papyrus of the Lake of Gennesaret	315
Mr. H. Seeley on a New Theory of the Skull and of the Skeleton.....	316
Proceedings of the Royal Society :—	
Mr. B. Stewart on the Specific Gravity of Mercury.	316
Proceedings of the Geological Society :—	
Mr. W. T. L. Travers on the mode of formation of certain Lake-Basins in New Zealand.....	318
Mr. R. Dawson on the occurrence of dead Littoral Shells in the bed of the German Ocean	318
Mr. T. F. Jamieson on the Glacial Phenomena of Caithness	318
On the Electrical Conductivity of Gases under feeble Pressures, by A. Morren	319
On St. Elmo's Fire, by Professor Frankland, F.R.S.	321
Historical Notice in reference to the retardation of the Earth's Velocity of Rotation, by Professor Fick	322
On Sea-levels, by D. D. Heath, Esq.	323
On the Axial Rotation of the Earth, by J. S. Stuart Glennie, M.A., F.R.A.S., &c.	323
On the relation between the Variation of Sun-spots and that of the Amplitude of Magnetic Oscillation, by Father Secchi ..	324

NUMBER CCX.—MAY.

M. W. Siemens on the Question of the Unit of Electrical Resistance	325
Mr. J. P. Cooke on the Aqueous Lines of the Solar Spectrum	337
Prof. Challis on the Motion of a small Sphere acted upon by the Undulations of an Elastic Fluid	343
M. L. Schwendler on the Galvanometer Resistance to be employed in testing with Wheatstone's Diagram.....	364
Prof. Young on the Completion of the Demonstration of Newton's Rule, and on a general property of derived Polynomials.	369
Mr. J. C. Moore on Glacial Submergence	372
Prof. Haughton on the Change of Eccentricity of the Earth's Orbit regarded as a Cause of Change of Climate	374
Dr. Matthiessen on the Question of the Unit of Electrical Resistance	376
Prof. Sylvester's Supplemental Note on the Analogues in Space to the Cartesian Ovals <i>in plano</i>	380
Prof. Tyndall on Calorescence	386
Proceedings of the Royal Society :—	
Prof. W. H. Miller on the Forms of Graphitoidal Silicon and Graphitoidal Boron	397
Proceedings of the Geological Society :—	
Mr. R. J. L. Guppy on the Tertiary Mollusca of Jamaica.	399
Mr. R. J. L. Guppy on Tertiary Echinoderms from the West Indies, and on Tertiary Brachiopoda from Trinidad	400
Dr. Young on the affinities of <i>Platysomus</i> , and on the Scales of <i>Rhizodus</i>	401

	Page
On a Gas-burner for Sounding large Tubes, by E. Reusch ..	401
Note on the Mechanical Equivalent of Light, by Moses G. Farmer	403
On the Composition of Forces, by Dr. Stevelly	404

NUMBER CCXI.—JUNE.

Mr. W. Huggins and Dr. W. A. Miller on the Spectra of some of the Fixed Stars. (With Two Plates.)	405
Mr. I. Todhunter on a Problem in the Calculus of Variations. .	425
Dr. A. Paalzow on the Heat of the Electric Spark	427
Archdeacon Pratt on the Fluid Theory of the Earth	430
Prof. Tyndall on Calorescence. (With a Plate)	435
Dr. Atkinson's Chemical Notices from Foreign Journals	451
Prof. Challis on the Fundamental Ideas of Matter and Force in Theoretical Physics	459
Proceedings of the Royal Society:—	
Mr. W. Huggins on the Spectra of some of the Nebulæ, with a mode of determining the Brightness of these Bodies	475
Dr. Everett's Experiments on the Flexural and Torsional Rigidity of a Glass Rod, leading to the determination of the Rigidity of Glass	476
Proceedings of the Geological Society:—	
Messrs. St. Vincent Lloyd, Delenda, and Décigala on the formation of a new island in the neighbourhood of the Kameni Islands	477
Mr. J. B. Jukes on the Carboniferous Slate of North Devon and South Ireland	477
Mr. W. B. Dawkins on the Fossil British Oxen	479
Commander G. Tryon on the formation of a new island in the neighbourhood of the Kameni Islands	479
Mr. T. McKenny Hughes on the Junction of the Thanet Sand and Chalk	479
Mr. W. Whitaker on the Lower London Tertiaries of Kent	480
Mr. W. Keene on the Brown Cannel Coal-seams at Colley Creek	481
The Rev. W. B. Clarke on the Oil-bearing Deposits in New South Wales	481
M. H. Bauerman on the Copper-mines of Michigan	482
On the Influence of the Electro-negative Elements upon the Spectra of the Metals, by M. Diacon	483
On the Determination of the Refractive Equivalent of the Elements, by M. A. Schrauf	483

NUMBER CCXII.—SUPPLEMENT.

Drs. A. Fick and J. Wislicenus on the Origin of Muscular Power.	485.
Mr. B. Stewart on the Solar Spectrum	503
Prof. J. A. Wanklyn on the Action of Carbonic Oxide on Sodium-ethyle	505

	Page
M. G. Neumayer on Aqueous Vapour and Terrestrial Radiation.	510
Mr. W. Huggins and Dr. W. A. Miller on the Spectra of some of the Fixed Stars	515
Mr. W. Huggins on the Spectra of some of the Nebulæ	523
Archdeacon Pratt on the Level of the Sea during the Glacial Epoch	532
Prof. W. Thomson on the Observations and Calculations required to find the Tidal Retardation of the Earth's Rotation	533
Proceedings of the Royal Society:—	
Mr. J. Evans on a possible Geological Cause of Changes in the Position of the Axis of the Earth's Crust	537
Proceedings of the Geological Society:—	
M. Fouqué on the Eruptions at the Kaimeni Islands	545
Mr. A. Tylor on the Upper and Lower Valley-gravels of part of England and France	546
Sir Philip de M. Grey Egerton on a new species of <i>Acanthodes</i> from the Coal-shales of Longton	546
Mr. H. Seeley on the Gravels and Drift of the Fenland ..	547
Prof. Harkness and Mr. H. Nicholson on the Geology of the Lake-country, and on the Lower Silurian Rocks of the Isle of Man	547
On the Law of the Union of Simple Substances, and on Attractions at Small Distances, by MM. Athanase and P. Dupré..	548
On a New Method of Measuring the Lengths of Luminous Waves, by Professor Stefan	550
On the Influence of Internal Friction in the Air on the Motion of Sound, by Professor Stefan	551
Index	552

PLATES.

- I. II. III. Illustrative of Sir David Brewster's Paper on the Bands formed by the Superposition of Paragenic Spectra produced by the Grooved Surfaces of Glass and Steel.
- IV. Illustrative of Prof. Tyndall's Paper on Calorescence.
- V. VI. Illustrative of Mr. W. Huggins and Dr. Miller's Paper on the Spectra of some of the Fixed Stars, and Mr. W. Huggins's on the Spectra of some of the Nebulæ.

ERRATA.

- Vol. 30. Page 410, lines 30 and 31, transpose the words "common oxygen" and "ozone."
- " — 439, line 12, for $\left(\frac{2500}{2499}\right)$ read $\left(\frac{2500}{2499}\right)^{80}$.
- Vol. 31. — 305, line 2 from bottom, for simple proportion to the latitude read simple proportion to the sine of the latitude.

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[FOURTH SERIES.]

JANUARY 1866.

I. *On the Law of Human Mortality expressed by a New Formula.*
By THOMAS ROWE EDMONDS, B.A. Cantab.*

THE love of life being the strongest of human passions, the most interesting of all laws ought to be the law which regulates the rate of decrement of human life from the time of birth until extreme old age.

Tables of mortality, in great variety, have been constructed for the purpose of exhibiting particular rates of decrement according to age appropriate to particular populations at different times. In nearly all these Tables there is exhibited a uniformly *decreasing* rate of decrement from the time of birth until the age of about 10 years, and a uniformly *increasing* rate of decrement from the age of about 15 years until the last age in the Tables. The appearances presented by all these Tables are such as to warrant the supposition that the rates of decrement in the period of childhood, as well as in the period of manhood, are functions of the age measured from birth or some other fixed time in the age of man. It will presently be seen that such supposition is well founded and in conformity with the facts given by observation. The rate of decrement of life will be found to be a certain function of the age, which is remarkable for simplicity and novelty.

If P_t be taken to represent the number living or surviving at the age t years, out of a given number born alive (P_0), the number dying in the $(t+1)$ th year of age will be $P_t - P_{t+1} = \Delta P_t$. The mean rate of decrement, or mean ratio of dying to living, during this year of age will be represented by ΔP_t divided by the mean number living throughout the year. Such mean number is $\frac{1}{2}(P_t + P_{t+1})$, or $P_{t+\frac{1}{2}}$ very nearly; so that the mean rate

* Communicated by the Author.

Phil. Mag. S. 4. Vol. 31. No. 206. Jan. 1866.

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of decrement in the $(t+1)$ th year of age is

$$\frac{2(P_t - P_{t+1})}{P_t + P_{t+1}} = \frac{2\Delta P_t}{P_t + P_{t+1}},$$

which may also be represented by $\frac{\Delta P_t}{P_{t+\frac{1}{2}}}$. The mean *annual* rate

of decrement in the $(t+1)$ th year being $\frac{\Delta P_t}{P_{t+\frac{1}{2}}}$, the mean *infinitesimal* rate of decrement during the same year of age will be $\frac{\Delta P_t}{q} \times \frac{1}{P_{t+\frac{1}{2}}}$, if q be taken to represent the infinitely great number of equal parts into which a unit of age is supposed to be divided so that the infinitely small given time dt multiplied by q is equal to unity.

The infinitesimal rate of decrement (or ratio of dying to living) will at the precise age (t) be represented by

$$\frac{P_t - P_{t+dt}}{P_t} = \frac{dP_t}{P_t} = d \cdot \log P_t$$

(since $\frac{dP}{P} = d \cdot \log P$, according to a well-known property of hyperbolic logarithms). That is to say, the infinitesimal rate of decrement at any age is identical with the differential of the hyperbolic logarithm of the number living or surviving at that age. Consequently, if the infinitesimal rate of decrement is a known function of the age, then the logarithm of the number living ($\log_e P_t$), being the sum of such infinitesimals, will also be a function of the age, which may be found by integration.

The infinitesimal rate of decrement at the precise age $(t+\frac{1}{2})$ years is $\frac{dP_{t+\frac{1}{2}}}{P_{t+\frac{1}{2}}}$, which is the differential of $\log P_{t+\frac{1}{2}}$. This may be taken to represent very nearly the mean infinitesimal rate of the $(t+1)$ th year of age, for which the expression $\frac{\Delta P_t}{q} \times \frac{1}{P_{t+\frac{1}{2}}}$ has already been found. On equating these two approximate values of the mean infinitesimal rate of decrement in the $(t+1)$ th year of age, it will ensue that $dP_{t+\frac{1}{2}} = \frac{\Delta P_t}{q}$ very nearly; i. e., the differential of the number living at the age $(t+\frac{1}{2})$ years is equal to the number of deaths in the $(t+1)$ th year of age divided by q , the infinitely great number of equal parts into which the unit one year is supposed to be divided.

When P is a function of the age (known or unknown), it will be found that all three of the following important quantities are equal to one another, very nearly, for any annual or quinquen-

nial interval of age, in the period of life between the ages of 15 and 80 years. These three quantities are (1) the mean ratio of the dying to the living throughout the interval, (2) the differential coefficient of the hyperbolic logarithm of the living at the middle point of the interval, and (3) the finite difference between the hyperbolic logarithms of the numbers living at the ages t and $(t+1)$. That is,

$$\frac{\Delta P_t}{P_{t+\frac{1}{2}}} = \alpha_{t+\frac{1}{2}} = \Delta \log_e P_t,$$

if $\alpha_{t+\frac{1}{2}}$ be used for differential coefficient $\frac{d \cdot \log P_{t+\frac{1}{2}}}{dt}$.

The first and second of the above quantities have already been shown to be equal to one another very nearly. The near coincidence in value of the second and third quantities will be obvious on consideration that, if the rate of decrement throughout the given unit interval of age varies continuously and equably from α_t at the beginning to α_{t+1} at the end of the interval, the total effect produced in the diminution of the number living (P_t) will be very nearly the same as the total effect in the same interval which would be produced by the rate of decrement $\alpha_{t+\frac{1}{2}}$ at the middle of the interval, assumed to be constant for the whole interval. On reference to Table V. hereunto annexed, it will be seen, for example, how slightly the three quantities above mentioned differ from perfect equality when the unit interval of age is five years. The remarkable property now mentioned, being possessed in common by all good Tables of mortality, is of great practical importance. For observations, hitherto supposed to give ratios of dying to living only, may henceforth be used as giving directly the finite differences of the logarithms of the living ($\Delta \log_e P_t$), which finite differences are the essential parts of the Tables of mortality sought to be constructed. In columns 5 and 6 of another Table (VII.) hereunto annexed, the reader may see how closely the values of $\Delta \log P$ in the English Life Table No 2 for males approach the approximate values obtained as above stated directly from observation.

When for a particular population the rates of decrement at every age are known, a Table of mortality may be constructed therefrom which will correctly represent the number living or surviving at the end of any entire number of years from birth, out of a given number born alive. Similarly, when the Table of mortality is given, and the number of survivors at every year of age is known, there may be deduced from such Table the rate of decrement for every age.

Observations for the purpose of determining the laws of mortality according to age of particular populations are made in one

or the other of the two ways just mentioned. The direct method, by observing the number of survivors at particular ages out of a given number living at birth-time, or out of a given number living at any other age, is applicable chiefly to selected classes distinct from the general population of a nation or district, such as classes of persons on whose lives annuities or assurances have been granted for money by trading companies or by governments.

The most common method of observation, by observing the rate of decrement at every age, is indirect, and is the only method used for determining the law of mortality, according to age, of the total population of a nation, or of the constituent parts of such population distinguished as village, town, or city population. Observations according to this method are made by periodical enumerations of the numbers living and the numbers who have died in annual, quinquennial, or decennial intervals of age. The ratios of the numbers dying to the numbers living (which are the rates of decrement of life) are thus obtained for every age. These rates being known, the consequent Table of mortality representing survivors at every year of age may be deduced by calculation.

The law of human mortality may be most simply expressed by means of the rates of decrement and the relation between these rates at different ages. If at two known ages the rates of decrement are α_0 and α_t , then, if the rate of decrement at one age is known, the rate of decrement at the other age may be found from the proportion

$$\frac{\alpha_t}{\alpha_0} = \left(\frac{a}{a+t} \right)^{\frac{1}{k}},$$

wherein t is the difference of age, a is a constant representing distance (in time or age) from a fixed point, which is the ideal zero of life or vital force, and $\frac{1}{k}$ is the hyperbolic logarithm of

10, and equal to 2.302585. There are two ideal zeros of human life—one belonging to the period of childhood, and the other to the period of manhood. The curve which indicates the law of decrease of human life consists of two branches—one on each side of the period of puberty. There apparently exists a short intermediate branch at or near the junction of the two others. The existence of such short branch may be due to the differences of age at which puberty is attained by different individuals.

The complete expression for the law of human mortality is contained in two similar formulæ,—one for the period of *increase* of vital force, extending from birth to the age of about 9 years; the other for the period of *decrease* of vital force, extending from

the age of about 12 years to the end of life. Both formulæ are similar to the formula which has been shown to represent the law, according to temperature, of the elastic force of steam of maximum density, and the formula which has been shown to represent the law of density of saturated steam (*Philosophical Magazine*, March and July 1865). All four formulæ are deduced—from a differential of the form following:—

$$d \cdot \log P = \alpha \left(1 + \frac{t}{a}\right)^{-\frac{1}{k}} dt.$$

In the two formulæ for human life, the quantity P represents survivors at the absolute age $(a+t)$ out of a unit of population existing at the absolute age (a) measured from one of the two zeros of life. The quantity α is the rate of decrement in a unit of time at the absolute age (a) whence t is measured, on the assumption that the infinitesimal rate of decrement at age a continues constant for the unit of time. The above equation for $d \cdot \log P$, on integration, yields

$$\text{com } \log P = -\frac{k\alpha a}{n} \left\{ 1 - \left(1 + \frac{t}{a}\right)^{-n} \right\},$$

wherein

$$n = \frac{1}{k} - 1 = 1.302585.$$

The most remarkable difference between the two new formulæ for human life, and the two formulæ for elastic force and density of saturated steam, will be found to consist in the relative positions of the ideal zeros of the forces of life and steam. In the case of steam the constant a (which indicates distance from the ideal zero) is very great, and marks a position of such zero far beyond the reach of observation, viz. 276° Centigrade below the temperature of melting ice. In the case of the two formulæ for human life, both of the ideal zeros are close at hand, and one of the two zeros may be passed by a living person. In the formula for the period of immaturity or childhood, the value of the constant a at birth-time is $2\frac{1}{4}$ years very nearly; that is to say, one of the zeros of life is an ideal point $2\frac{1}{4}$ years before the time of birth. In the formula for the period of maturity or manhood, the place of the ideal zero of vital force is at the age 102 years from birth-time nearly.

When the places of the two ideal zeros of life have been determined (say at $-2\frac{1}{4}$ years and at $+102$ years from birth), there remains only to be determined the point of meeting of the two periods, and the rate of mortality or decrement of life common to both periods at the point of junction. According to the

observations made on the population of England, the point of junction of the two periods is very near the age 9 years and 4 months from birth-time. But in seven out of eight of the Tables of comparison hereunto annexed, the column which exhibits the results of the new formula is taken from a Table in which the period of childhood is assumed to terminate at the age of 9 complete years, and the period of manhood to commence at 12 years of age. The rate of mortality or rate of decrement of life, in the intermediate period from 9 to 12 years of age, has been assumed to be constant and at a minimum of $\cdot 0055$ per annum, or $\frac{\cdot 0055}{q}$ in time dt .

The two branches of the curve indicating the law of decrease of human life (before 9 and after 12 years of age) bear a simple relation to one another. If the abscissæ of the two curves, between the zeros of life and the ages of minimum mortality, be divided into the same number of equal parts, the logarithms of the corresponding ordinates are to one another in a fixed proportion, that of 1 to 8 very nearly. If the period of childhood, extending from $-2\frac{1}{4}$ years to $+9$ years of age, be divided into 15 equal parts of 9 months each, and the period of manhood, extending from 12 to 102 years of age, be divided into the same number of equal parts, each of 6 years, in that case the rate of mortality will *decrease* in any one of the 15 equal intervals into which the period of childhood is divided, exactly eight times as fast as the rate of mortality in the corresponding interval of manhood *increases*. The vital force being inversely as the rate of mortality, it ensues that the *increase* of vital force in passing through any interval of 9 months in the period of childhood is exactly counterbalanced by a *decrease* of vital force in passing through the corresponding interval of 6 years in the period of manhood. The existence of the relations now mentioned between the two branches of the curve of life, renders it unnecessary to make separate calculations by the new formula for the values of $\log P$ in both curves; for when $\log P$ is known for one curve, it is known for the other curve also, by the use of a constant multiplier, as may be seen on reference to Table III. hereunto annexed. On reference to Table II. hereunto annexed, it will also be seen, at each of 15 intervals of age, how nearly the rates of decrement in the period of childhood resemble those of corresponding ages in the period of manhood, according to the English Life Table No. 3 for males.

The only good observations in the rates of decrement, according to age, prevailing in the general population of a nation are those which have been made in Sweden and in England. The Swedish observations commence with the year 1749; the English

observations with the year 1813. Both series of observations, with insignificant interruptions, have been continued to the present year (1866).

The method of observation in both series has been substantially the same. In Sweden and in England there have been made periodical enumerations of the living and dying, in quinquennial intervals of age, from the age of 5 years upwards, and in annual intervals for ages below five years. The Swedish observations have been satisfactorily made, judging from the internal evidence of correctness afforded by uniformity of increase of ratios of dying to living for consecutive quinquennial intervals of age. Similar success has not attended the English observations for quinquennial intervals of age; for the several indicated rates of mortality for even quinquennial intervals of age ending at the ages 30, 40, 50, 60, and 70 years are nearly as great as the several indicated rates of mortality for odd quinquennial intervals of age, five years greater, ending at the ages 35, 45, 55, 65, and 75 years. This will be seen on reference to column 5 in Table VI. hereunto annexed. The amount of error is considerable, and remains unaccounted for. The English Life Tables Nos. 2 and 3 are founded on these observations, corrected by the assumption that the errors are in opposite directions and neutralize one another, the true rate of mortality being assumed to be halfway between consecutive contradictory quinquennial rates. The publication of the numbers dying in quinquennial intervals of age ceased with the year 1850, the intervals of age now adopted for publication being decennial for ages exceeding 25 years.

On comparison of the rates of mortality for quinquennial intervals of age, of the male population of Sweden and England (the former for 61 years, and the latter for 7 years), it will be found that, according to the general average for all ages exceeding 25 years, the Swedish rates exceed the English rates at the same ages in the proportion of 120 to 100. When, however, the comparison is separately made for even and for odd quinquennial intervals of age, it is found that the Swedish rates exceed the English rates in the proportion of 109 to 100 for *even* quinquennial intervals of age, and in the proportion of 131 to 100 for *odd* quinquennial intervals of age.

According to all observations on human mortality, there exists a short period near the age of 12 years at which the rate of mortality is at a minimum and apparently stationary. In the English observations this period extends from age 10 to age 15 years, in the Swedish observations from age 10 to age 20 years. If the English observations at the quinquennial interval of age 15 to 20 are to be relied upon, it would ensue that the propor-

tion of persons whose period of immaturity is prolonged beyond the average period is considerably greater in the population of Sweden than in the population of England. There is, however, some reason to doubt the correctness of the English observations at the age mentioned. For errors (in excess of the true rate of mortality) are admitted to exist at the quinquennial intervals of age ending at 30, 40, 50, &c. years; whilst the rates given by observation for the quinquennial interval of age ending at 20 years are assumed to be correct. A correction in diminution of the apparent rate of mortality has been applied in the former case; and no reason has been assigned for not applying a similar correction to the apparent rate for the quinquennial interval of age ending with 20 years.

The short intermediate branch of the curve of human life at which the rate of mortality is stationary, or apparently stationary, will be represented by a logarithmic curve of which the equation is $P_t = e^{-at}$, or $\log_e P_t = -at$. This equation may be deduced from the general formula, which is

$$\begin{aligned}\log_e P_t &= -\frac{aa}{n} \left\{ 1 - \left(1 + \frac{t}{a} \right)^{-n} \right\} \\ &= -at \left\{ 1 - \frac{n+1}{2} \frac{t}{a} + \frac{n+1}{2} \cdot \frac{n+2}{2 \cdot 3} \frac{t^2}{a^2} - \&c. \right\} \\ &= -at \left\{ 1 - 1 \cdot 151292 \frac{t}{a} + 1 \cdot 267414 \frac{t^2}{a^2} - 1 \cdot 363290 \frac{t^3}{a^3} + \&c. \right\}.\end{aligned}$$

On making a infinite all the terms vanish except the first, leaving $\log_e P_t = -at$, or $P_t = e^{-at}$. If $t=1$ and $a=.0055$, then

$$P_1 = e^{-a} = e^{-.0055} = 10^{-.0055 \times k} = .9945150 = 1 - r = 1 - .0054850.$$

That is, the constant ratio of decrease in one year of the living between the ages of 9 and 12 years is the number whose hyperbolic logarithm is $-.0055$.

The distinguishing and very useful property of the new formula consists in the simple relation which connects together the ratios of the finite differences $\Delta \log P$. If the unit interval of age is small and does not exceed 6 years in the period of manhood, it will be found (using common logarithms) that

$$\Delta \log \Delta \log P_{t+1} - \Delta \log \Delta \log P_t = -\frac{1}{a+t}$$

very nearly, $a+t$ representing distance of the ideal zero of life from the middle point common to the two intervals com-

pared, of $\Delta \log P$. By the aid of this property, Tables of mortality may be constructed for all ages without direct use of the new formula, except for determining one value of $\Delta \log P_t$ for a particular age (t) at a known distance from the zero of life. When one value of $\log \Delta \log P_t$ has been obtained, all other values of $\log \Delta \log P_t$ may be obtained by successive additions or subtractions of the quantities $\frac{1}{a+t}$ extracted from a Table of reciprocals. The property mentioned will be found illustrated in Table IV. hereunto annexed, wherein the unit interval of age in the period of manhood has been assumed to be 6 years.

The distinguishing property, just mentioned, of the new formula is dependent on, and deducible from, the law which connects together the differential coefficients $\frac{d \cdot \log P}{dt}$ and the logarithms of such differential coefficients. It has already been shown that such differential coefficients are represented by

$$\alpha_t = \alpha \left(1 + \frac{t}{a}\right)^{-\frac{1}{k}}.$$

Taking the ratio of two consecutive differential coefficients at unit intervals,

$$\frac{\alpha_{t+1}}{\alpha_t} = \frac{d \cdot \log P_{t+1}}{d \cdot \log P_t} = \left(\frac{a+t+1}{a+t}\right)^{-\frac{1}{k}},$$

and taking com logs of both sides of above equation,

$$\begin{aligned} \text{com log } \alpha_{t+1} - \text{com log } \alpha_t &= -\frac{1}{k} \{ \text{com log } (a+t+1) - \text{com log } (a+t) \} \\ &= -\{ \text{hyp log } (a+t+1) - \text{hyp log } (a+t) \}, \end{aligned}$$

the above equation also gives in hyp logs,

$$\begin{aligned} \text{hyp log } \frac{\alpha_{t+1}}{\alpha_t} &= -\frac{1}{k} \text{hyp log } \frac{a+t+1}{a+t} = -\frac{1}{k} \text{hyp log } \left(1 + \frac{1}{a+t}\right) \\ &= -\frac{1}{k} \left\{ \frac{1}{a+t} - \frac{1}{2(a+t)^2} + \frac{1}{3(a+t)^3} - \&c. \right\} \\ &= -\frac{1}{k} \times \frac{1}{a+t} \text{ at the limit.} \end{aligned}$$

That is, when the intervals of age are indefinitely small, the differences of the hyperbolic logarithms of the differential coefficients of $\log P_t$ are $= -\frac{1}{k} \times \frac{1}{a+t}$, and consequently the differ-

ences of the common logarithms of the same differential coefficients are $= -k \times \frac{1}{k} \times \frac{1}{a+t} = -\frac{1}{a+t}$.

The value of the common logarithm of P may be most conveniently calculated in the two periods of childhood and manhood from the formula

$$\log P_{15-t} = -\frac{15 \cdot k\alpha}{n} \left\{ \left(\frac{15}{15-t} \right)^n - 1 \right\},$$

α being the minimum rate of mortality in both periods at the extreme distance, fifteen units, from the ideal zero of life, and the value of α for the unit of 6 years in the period of manhood being .0330, and for the unit of 9 months in the period of childhood being .004125. The age (t) measured from the limit of minimum mortality is negative.

From the above formula may be obtained the formula for

$$\begin{aligned} \Delta \log P_{15-t} &= \log \frac{P_{15-t-1}}{P_{15-t}} \\ &= -\frac{15 \cdot k\alpha}{n} \left(\frac{15}{15-t} \right)^n \left\{ \left(1 - \frac{1}{15-t} \right)^{-n} - 1 \right\}, \end{aligned}$$

which, expressed in a series, becomes

$$\Delta \log P_{15-t} = -k\alpha \left(\frac{15}{15-t} \right)^{\frac{1}{k}} \left\{ 1 + \frac{B_1}{15-t} + \frac{B_2}{(15-t)^2} + \&c. \right\}.$$

The coefficients B_1 , B_2 , B_3 , &c. belong to the general formula, and their value has already been given in a preceding page.

The total population of every civilized nation is divisible into three important parts, suffering different rates of mortality at the same ages. The village population suffers a lower rate of mortality than the town population, and the town population lower than the city population. The proportional rates of mortality for village, town, and city populations may be estimated as 5, 6, and $7\frac{1}{2}$ respectively.

The fact of the total population of a nation consisting of three large classes suffering different rates of mortality at the same ages, will exercise a disturbing effect on the manifestation of the true law of mortality in adult life. At early ages, say from 20 to 45 years, the national Table for the total population will represent the mean rate derived from due proportions of village, town, and city populations. At middle age, say from 45 to 65 years, the national Table will represent the mean rate of a total population consisting of more than a due proportion of village, and less than a due proportion of city population, through the

more rapid disappearance of the members of the classes suffering the higher mortality. At the later ages of life, say for ages above 65 years, the city population will nearly all have disappeared, and the national Table will be a representation of the mortality of a population consisting, for the greater part, of villagers. The mortality in advanced life will always be less than that indicated by the true law derived from observations on middle life. The apparent law is derived from the true law subjected to continual changes through the increase, as age advances, of the proportion of lives of a superior class. At the later ages of life an empirical law of mortality, such as is hereafter mentioned, may represent the apparent facts better than they can be represented by the true law of mortality applicable to classes formed of individuals equal in vital force at all ages passed through.

In Table I. hereunto annexed, is given the Life Table No. 3 for the male population of England, which was published in the year 1864 by authority of the Registrar-General. This Table has been offered and may be accepted as a correct representation of the decrement of life, at all ages, observed during the 17 years 1838-54. The name of the constructor, Dr. William Farr, is a sufficient guarantee for fidelity and correctness in converting or transmuting the irregular numbers given by observation into their just equivalents expressed by the equable flow of a well-adjusted life Table. In this Table (I.) is expressed the number living or surviving at different ages in relation to 1000 living at the age 12 years adopted as a basis. In adult life the numbers living have been extracted from the original Table for every fifth year of age. From the age of 1 year to 12 years the intervals are annual. At ages under 1 year, the numbers living have been extracted for the ages 0, one month, two months, three months, and six months,—these comprising all the numbers which have been obtained by direct observation.

In Table I. a column has been added in which is contained a life Table deduced from the new formula, and intended for comparison with the new English Life Table for males in the adjoining column. On inspection it will be seen that for all ages above one month and less than 12 years, the new formula yields results which are all but identical with the results of observation as indicated by Dr. Farr. In devising and constructing theoretical Tables of mortality, the greatest difficulty has generally been acknowledged to consist in finding any general law which could satisfactorily represent the very rapidly changing mortality in early infancy. That such a law representing the facts in early infancy should be contained in the new formula, is the strongest attainable confirmation of the truth and applicability of

this formula. The coincidence, at this period of life, which establishes the truth of the new formula, establishes at the same time the great credit for accuracy due to the English observations at periods of life in which the returns are not affected by the disposition of the people to understate or overstate their ages.

Comparing the observations of Sweden in the present century with the observations of Sweden and Finland in the previous century, there is found in the more recent observations a considerable *decrease* in the mortality at all ages under 20 years, and a considerable *increase* in the mortality at all ages above 30 years. Both sets of observations are in accordance with the new formula, but the place of the ideal zero of life in the period of manhood appears to have receded in the present century three or four years.

The place of the ideal zero of life for the period of childhood has been seen to be at $2\frac{1}{4}$ years, or 27 months before the time of birth. That is to say, when units of 9 months are taken, the place of such zero is distant *three* of such units from the time of birth, or *two* of such units from the time of conception.

It has been found, through observations collected by Dr. Granville at lying-in hospitals, that out of about 450 pregnant women 150 had suffered miscarriage, the average proportion being one miscarriage to two births alive, and that the number of miscarriages was much greater in the earlier months of pregnancy than in the later months, the remaining 300 pregnant women having suffered no miscarriages. The statements thus made accord well enough with the assumption that the miscarriages during the first seven months of pregnancy are represented by the deaths which would occur if the law of mortality after birth-time were continued backwards to the time of conception. On this assumption the mortality at the commencement of uterine life, or immediately after the time of conception, would be 56·9 per cent. per annum, the corresponding mortality at birth-time being 22·4, and at age 9 years being 0·55 per cent. per annum. This result, leading to the conclusion that 56·9 per cent. is the maximum rate of mortality capable of being measured or regulated by the new formula, is indirectly confirmed by the last English life Table for males, wherein it is stated that 55 per cent. per annum is the highest rate of mortality observed at the most advanced age.

According to the new formula, extending over two periods, bounded as stated by the ideal zeros of life at $-2\frac{1}{4}$ and $+102$ years of age (with a mortality of 0·55 per cent. from the age of 9 to the age of 12 years, common to both periods), the rate of mortality at birth-time is equal to the mortality at the age 84

years, and the mortality 9 months before birth is equal to the mortality at the age 90 years. It would hence follow that mortality above the age of 90 years is not measurable by the new formula, because the mortality before the time of conception is not so measurable. It may be further stated that, according to the English Life Table for males, the mortality in the first month from birth-time is 64 per cent. per annum. This number does not differ much from 56.9, the maximum mortality given by the formula. So high a rate of mortality as 64 per cent. per annum in the first month, ought not to excite surprise; for in the deaths of the first month from birth-time, it may be presumed, are included the deaths arising from miscarriage and premature birth in the eighth and ninth months from conception.

There is some ground for believing that 9 months, the period of gestation in the human female, is the atomic indivisible unit of age in the period of childhood. It has been shown that, according to the new formula, the mortality at any age varies inversely as $(a+t)^{\frac{1}{k}}$ or $(1+t)^{\frac{1}{k}}$, when reduced to its simplest form, $1+t$ representing absolute age measured from the ideal zero of life. This quantity $(1+t)$ is apparently contained in the new formula for the sake of its convertibility from numbers into logarithms by the formula

$$\log(1+t) = t - \frac{t^2}{2} + \frac{t^3}{3} + \&c.$$

The lowest whole number which can represent t in the above formula is unity; that is to say, $\log(1+1)$ or $\log 2$ is used at the first step which can be made in the application of the new formula in proceeding from the ideal zero to the ages 1, 2, 3, &c. The mortality immediately after the time of conception is the very earliest that can be measured by the formula, and then $1+t$ is represented by $1+1$ or by 2. It being known that the absolute age of conception is 18 months from the ideal zero of life, it ensues that the 18 months contain two units each of 9 months.

On inspection of Table VI. (hereunto annexed), in which are presented in a condensed form the results of all the chief observations on the mortality, according to age, of the male populations of Sweden and England, it will be seen that the new formula fails to express the apparent law at ages exceeding 85 years. According to the new formula, the survivors at the age 85 years disappear more rapidly with age than they are supposed to do by any other law. According to observation, both in Sweden and England, the average rate of mortality in the decennial interval of age from 85 to 95 years is just twice as great as the average rate of mortality in the decennial interval of age

from 75 to 85 years. The appearances presented are not, however, inconsistent with the truth of the new formula. To reconcile the new formula with the facts, it will suffice to make the reasonable supposition that the position of the ideal zero of life in the period of manhood is variable in the different classes constituting the total populations observed, say from the age 105 years for village population to the age 95 years for city population.

In addition to the new formula, by which the law of human mortality, according to age, may be expressed, there exists an old formula by means of which the mortality at all ages may be equally well expressed—with this difference only, that the new formula fails for ages exceeding 85 years, whilst the old formula fails for ages under 2 years. Both formulæ may be said to express laws of mortality; but the law contained in the old formula is to be regarded as no more than an empirical law secondary to, and dependent on, the true law exhibited by the new formula.

According to the empirical law contained in the old formula, the rate of mortality at ages exceeding 55 years *increases* in a constant ratio of 1.08 per cent. for every additional year of age. From age 12 to age 55 the constant ratio of increase is 1.03 per cent. From age 8 to age 12 the rate of mortality is constant and at a minimum. From the time of birth to the age of 8 years the annual ratio of *decrease* of mortality, according to the old formula, is 67.6 per cent. From this empirical law is deducible the differential equation

$$d \cdot \log P_t = -\alpha p^t dt,$$

which on integration yields the equation following,

$$\text{com log } P_t = \frac{k^2 \alpha}{\lambda p} (1 - p^t),$$

λp having three different values, $-.17$, $+.0128$, and $+.0333$ in the three periods of infancy, florescence, and senescence.

The above formula for $\text{com log } P_t$ was first published in the year 1832. The formula with the three values of p above given, was used for the construction of three theoretical Tables distinguished as exhibiting village, mean, and city mortality respectively. Accompanying these theoretical Tables was published a full collection of derivative Tables, consisting chiefly of values of annuities on single and joint lives at various ages and various rates of interest*. The minimum rates of mortality respectively adopted in the three Tables at the age of 10 years were .005, .006, and .0075 per annum.

The theoretical Table of "*Village Mortality*" indicates rates of mortality which at all ages agree very closely with the rates

* Life Tables, by T. R. Edmonds, B.A. (1832).

indicated by the "Carlisle Table" of Mr. Joshua Milne, constructed in 1815. The latter Table was deduced from observations of Dr. Heysham, on the general population of both sexes in the town of Carlisle during the nine years ending with 1787, and is now commonly used in England as the measure of the mortality according to age of selected lives. The Table previously used for that purpose was one specially applicable to selected lives, which was deduced by M. Deparcieux from observations on the mortality, according to age, of persons of both sexes selected as nominees in French Tontines. The Tontine Life Table of M. Deparcieux, as well as his Life Tables for various communities of monks and nuns, was published in the year 1746. Mr. Milne writes thus of these Life Tables of M. Deparcieux:—"They are among the most curious and correct, and of the best authority that have yet been published." The law of mortality for select life differs considerably from that for general life, especially for the period extending from the age 20 to the age 50 years. The mortality increases with the age during this period much more slowly in select life than it does in general life. The following Table of "Expectation," or mean duration of future life, in years, at seven different ages, according to the three Tables above referred to, will show how greatly these Tables resemble one another:—

Age in years	20.	30.	40.	50.	60.	70.	80.
Theoretical village.....	41·4	34·3	27·7	21·0	14·5	9·2	5·4
Milne, Carlisle	41·5	34·3	27·6	21·1	14·3	9·2	5·5
Deparcieux, Tontine ...	40·2	34·0	27·5	20·4	14·2	8·7	4·7

The theoretical Table of "*Mean Mortality*," which is often referred to in the annexed Tables, indicates rates of mortality which, at all ages exceeding 10 years, are in near agreement with the rates indicated by a Table constructed by Mr. Milne to represent the mortality, according to age, of the population of Sweden and Finland, without distinction of sex, during the 20 years ending with 1795. A similar Table, for the same population during the 21 years ending with 1775, was constructed by Dr. Richard Price in the year 1783. The subjoined Table of "Expectation" at seven different ages will show the relation between the theoretical mean mortality Table and the Swedish Tables of Dr. Price and Mr. Milne.

Age in years	20.	30.	40.	50.	60.	70.	80.
Theoretical mean	38·6	31·9	25·5	19·2	13·1	8·2	4·7
Price's Sweden	38·0	31·2	24·7	18·5	12·6	7·7	4·3
Milne's Sweden	39·0	32·1	25·5	19·0	12·8	8·0	4·8

The theoretical Table of "*City Mortality*" above mentioned is represented nearly by the mortality, according to age, observed in the male population of English towns or cities of the first magnitude, with the exception, however, of Liverpool and Manchester, which towns form a special class distinguished for excessively great mortality. The same theoretical Table is also represented at ages exceeding 40 years by the mortality, according to age, observed by M. Duparcieux among monks in the convents of Paris and its environs. According to M. Duparcieux's Tables, the mortality of monks resident in Paris exceeded the mortality at the same ages of Tontine nominees, also resident in Paris, in the proportion of 3 to 2 nearly, being the proportion in which the rates of the "*City*" Table exceed those of the "*Village*" Table.

The general population of a nation consists chiefly of persons earning their subsistence by bodily labour, and by different degrees of bodily labour, according as they reside in villages, towns, or cities. The law of mortality, according to age, of the general population has not necessarily any close connexion with the law of mortality of that portion of the community which is abundantly supplied with the necessaries of life and is removed from the necessity of earning its subsistence by bodily labour. Moreover it would appear, from M. Deparcieux's observations, that in the portion of the community mentioned the general rate of mortality may probably be reduced 30 per cent. by selecting individuals of superior vital force to form a class of government annuitants.

The competition of the English with the Swedish observations for the credit of supplying the best measure of the general mortality, according to age, of the population of European nations did not commence until the year 1835. In that year were first published government returns of the population of England, in which the numbers living and the numbers who had died were distributed in decennial intervals of age when the age exceeded 20 years, and in quinquennial intervals of age when the age was less than 20 years. There was no statement at the same time published of the ratios of the dying to the living at the different ages. The comparison of the numbers dying and living at each age, and the determination of the law of mortality prevailing in England and the counties of England during the 18 years ending with 1830, were left to the investigation of private individuals who might choose to undertake the task. In the same year (1835)* the present writer was the first to communicate to the public the law of mortality, according to age, of the population of England.

* *Lancet*, December 1835, pp. 364, 408.

TABLE I.—Comparison of the English Life Table No. 3 for Males (deduced by Dr. Farr from observations of 17 years, 1838–54), with the Life Table yielded by the New Formula, and with a Theoretical Life Table published in the year 1832.

Age from birth.	Logarithm of living, according to new formula.	Number living according to new formula.	English life Table No. 3, for males.	Mean mortality Table of 1832.
Years.				
0	3·1544013	1427	1465	1465
$\frac{1}{12}$	·1466346	1402	1388	
$\frac{1}{6}$	·1394813	1379	1361	
$\frac{1}{3}$	·1328743	1358	1342	
$\frac{1}{2}$	·1157890	1305	1296	
1	·0905129	1232	1225	1298
2	·0598486	1148	1147	1197
3	·0422084	1102	1106	1132
4	·0308977	1074	1080	1091
5	·0230993	1055	1060	1064
6	·0174354	1041	1046	1046
7	·0131573	1031	1035	1034
8	·0098253	1023	1025	1026
9	·0071658	1017	1017	1019
10	·0047772	1011	1011	1013
11	·0023886	1005	1005	1006
12	3·0000000	1000	1000	1000
15	2·9925487	983	986	980
20	·9787240	952	955	944
25	·9628124	918	915	904
30	·9443304	880	872	860
35	·9226377	837	827	811
40	·8968692	789	779	758
45	·8658292	734	726	701
50	·8278200	673	668	640
55	·7803567	603	600	576
60	·7196583	524	522	502
65	·6397053	436	432	410
70	·5303434	339	327	305
75	·3731253	236	217	197
80	·1310138	135	118	104
85	1·7183120	52	48	41
90	0·8877140	8	14	10
95	2·5694870	0	2	1

TABLE II.—Annual Rates of Mortality, according to the English Life Table No. 3 for Males, at intervals of 9 months in Childhood, and 6 years in Manhood, compared with the annual rates at the same ages according to the New Formula, and according to a National Theoretical Table published in 1832.

Age from zero of life, in units of 9 months or 6 years.	Age from birth.		Annual rate by new formula in both periods.	England. Males. 17 years, 1838-54.		Mean mortality Table of 1832.
	In manhood.	In childhood.		In period of childhood.	In period of manhood.	
	Years.	Years.				
15	12	9·00	·0055	·0070	·0049	·0064
14	18	8·25	·0064	·0080	·0065	·0076
13	24	7·50	·0076	·0092	·0089	·0091
12	30	6·75	·0092	·0104	·0100	·0108
11	36	6·00	·0112	·0122	·0115	·0129
10	42	5·25	·0140	·0147	·0137	·0154
9	48	4·50	·0178	·0180	·0171	·0184
8	54	3·75	·0234	·0224	·0231	·0219
7	60	3·00	·0318	·0296	·0320	·0332
6	66	2·25	·0454	·0422	·0488	·0525
5	72	1·50	·0690	·0668	·0786	·0832
4	78	·75	·1154	·1120	·1265	·1318
3 $\frac{1}{3}$	82	·25	·1756	·1564	·1710	·1791
3	84	·00	·2237	·6450	·1976	·2088
2	90	— ·75	·5692	·2976	·3308
1	96	— 1·50	2·8081	·4324	·5241

TABLE III.—Values of log P and $\Delta \log P$ at each of fifteen intervals of age in the periods of Childhood and Manhood respectively, obtained from the same formula,

$$\log P = -\frac{k\alpha \times 15}{n} \left\{ \left(\frac{15}{15-t} \right)^n - 1 \right\},$$

the values of α being ·004125 or ·0330, according as units of 9 months or units of 6 years are involved.

Age from limits in units of 9 months or 6 years.	In period of childhood.			In period of manhood.		
	Age from birth.	Log P.	$\Delta \log P.$	$\Delta \log P.$	Log P.	Age from birth.
	Years.					Years.
15	9·00	·0000000	·0019399	·0155187	·0000000	12
14	8·25	·0019399	·0022871	·0182984	·0155187	18
13	7·50	·0042270	·0027317	·0218526	·0338171	24
12	6·75	·0069587	·0033110	·0264876	·0556697	30
11	6·00	·0102697	·0040844	·0326755	·0821573	36
10	5·25	·0143541	·0051465	·0411704	·1148328	42
9	4·50	·0195006	·0066541	·0532338	·1560032	48
8	3·75	·0261547	·0088878	·0711047	·2092370	54
7	3·00	·0350425	·0123803	·0990406	·2803417	60
6	2·25	·0474228	·0182424	·1459381	·3793823	66
5	1·50	·0656652	·0291083	·2328676	·5253204	72
4	·75	·0947735	·0524621	·4196970	·7581880	78
3	·00	·1472356	·1168005	·9344010	1·1778850	84
2	— ·75	·2640361	·4175204	3·3401550	2·1122860	90
1	— 1·50	·6815565			5·4524410	96

TABLE IV.—Values of the finite differences $\Delta \log \Delta \log P$, according to the New Formula, shown to be equal very nearly to the reciprocals of the distances of the middle points from the ideal zeros of life, even when the unit interval of age is so large as 6 years in the period of Manhood.

Interval of age, in units of 9 months or 6 yrs. from zeros of life.	$\Delta \log P$ (in manhood).	Log $\Delta \log P$.	$\Delta \log \Delta \log P$.	Reciprocal of last number, or $\Delta \log \Delta \log P$.	Distance from limits, in units of 9 months or 6 years.	Age from birth.	
						In childhood.	In manhood.
						Years.	Years.
15-14	·0155187	2·1908553	·0715578	13·975	14	8·25	18
14-13	·0182984	·2624131	·0770900	12·972	13	7·50	24
13-12	·0218526	·3395031	·0835395	11·970	12	6·75	30
12-11	·0264876	·4230426	·0911797	10·967	11	6·00	36
11-10	·0326755	·5142223	·1003628	9·964	10	5·25	42
10-9	·0411704	·6145851	·1116024	8·960	9	4·50	48
9-8	·0532338	·7261875	·1257108	7·955	8	3·75	54
8-7	·0711047	·8518983	·1439149	6·948	7	3·00	60
7-6	·0990406	·9958132	·1683555	5·940	6	2·25	66
6-5	·1459381	1·1641687	·2029404	4·927	5	1·50	72
5-4	·2328676	·3671091	·2558268	3·909	4	·75	78
4-3	·4196970	·6229359	·3475974	2·877	3	·00	84
3-2	·9344010	·9705333	·5532333	1·807	2	—·75	90
2-1	3·3401550	0·5237666					

TABLE V.—Showing, for any quinquennial interval of age above 15 and less than 80 years, that there is no difference, appreciable by observation, between the three quantities following,—viz. the quinquennial ratio of the dying to the living, the hyperbolic logarithm of the quinquennial rate of decrement at the middle of the interval, and the difference $\Delta \log_e P$ between the hyperbolic logarithms of the numbers living at the beginning and at the end of such interval.

Interval of age.	One-fifth part of quinquennial ratio of dying to living.	One-fifth part of quinquennial rate of decrement at middle of interval.	One-fifth part of $\Delta \log P$.	
			In hyp logs.	In com logs.
Years.				
15-20	·00685	·00684	·00685	·00297
20-25	·00787	·00786	·00787	·00342
25-30	·00912	·00911	·00912	·00396
30-35	·01067	·01067	·01068	·00464
35-40	·01265	·01264	·01266	·00550
40-45	·01519	·01517	·01520	·00660
45-50	·01852	·01850	·01855	·00806
50-55	·02302	·02300	·02307	·01002
55-60	·02926	·02924	·02935	·01275
60-65	·03824	·03823	·03841	·01668
65-70	·05174	·05178	·05210	·02263
70-75	·07321	·07344	·07407	·03217
75-80	·10993	·11091	·11228	·04876
80-85	·17882	·18333	·18684	·08115

Note.—In the complete theoretical Table for annual inter-

vals of age, used above for illustration, the zero of life has been taken at 103 years; $a=94$ years, and $\alpha_{94}=.0055$. The theoretical Table elsewhere used for illustration is incomplete, the values of P having been calculated for every fifth and sixth year of age only. In that Table, as elsewhere stated, the zero of life is fixed at 102 years of age, whilst $a=90$, and $\alpha_{90}=.0055$.

TABLE VI.—Showing for the total Male Populations of Sweden and England respectively the annual ratios of the Dying to the Living in quinquennial intervals of age, and the sums of such ratios; which ratios, being also hyperbolic logarithms, represent very nearly one-fifth part of the finite differences $\Delta \log P$ between the hyperbolic logarithms of the numbers living at the beginning and at the end of the interval of age observed.

Interval of age.	Sweden and Finland.		Sweden.	England.	Theoretical, from	
	21 years, ending 1775.	20 years, ending 1795.	20 years, ending 1840.	7 years, ending 1844.	New formula.	Formula of 1832.
Years.						
0-1	·2852	·2377	·2141	·2057	·1495	·1205
1-3	·0578	·0577	·0396	·0512	·0560	·0688
3-5	·0290	·0285	·0160	·0220	·0221	·0313
5-10	·0147	·0143	·0078	·0093	·0085	·0099
10-15	·0076	·0068	·0049	·0051	·0056	·0065
15-20	·0072	·0068	·0049	·0071	·0064	·0075
20-25	·0095	·0090	·0079	·0092	·0073	·0087
25-30	·0103	·0106	·0095	·0098	·0085	·0100
30-35	·0121	·0117	·0124	·0097	·0100	·0116
35-40	·0129	·0126	·0145	·0126	·0118	·0135
40-45	·0175	·0160	·0180	·0125	·0143	·0156
45-50	·0207	·0192	·0217	·0173	·0175	·0181
50-55	·0275	·0240	·0270	·0184	·0218	·0210
55-60	·0325	·0300	·0350	·0297	·0278	·0274
60-65	·0450	·0439	·0461	·0332	·0366	·0402
65-70	·0627	·0663	·0668	·0597	·0500	·0588
70-75	·0931	·0928	·1035	·0741	·0717	·0858
75-80	·1244	·1325	·1538	·1271	·1090	·1250
80-85	·1943	·1864	·2446	·1753	·1816	·1816
85-90	·2516	·2467	·3300	·2833	·3600	·2623
above 90	·4012	·3352	·4326	·3688	·3910
15-25	·0167	·0158	·0128	·0163	·0137	·0162
25-35	·0227	·0223	·0219	·0195	·0185	·0216
35-45	·0304	·0286	·0325	·0251	·0261	·0291
45-55	·0482	·0432	·0487	·0357	·0393	·0391
55-65	·0775	·0739	·0811	·0629	·0644	·0676
65-75	·1558	·1591	·1703	·1338	·1217	·1446
75-85	·3187	·3189	·3984	·3024	·2906	·3066
25-45	·0531	·0509	·0544	·0446	·0446	·0507
45-65	·1257	·1171	·1298	·0986	·1037	·1067
65-85	·4745	·4780	·5687	·4362	·4123	·4512
25-85	·6533	·6460	·7529	·5794	·5606	·6086

TABLE VII.—True values and approximate values of $\Delta \log_{10} P$, for intervals of 10 years and 20 years of age, according to the New and the Old Formula. Also approximate values of $\Delta \log_{10} P$ obtained by observation, compared with the $\Delta \log_{10} P$ of the English Life Table No. 2 for Males.

Interval of age.	New formula.		English life Tables for males.		Obs. of 7 years ending 1844.	Mean mortality Table of 1832.	
	True, $\Delta \log P$.	Approximate, $=5k\alpha t + \frac{1}{2}$.	No. 3. 17 years, ending 1854, $\Delta \log P$.	No. 2. 7 years, ending 1844, $\Delta \log P$.	Approximate, $\Delta \log P = 5k\alpha t + \frac{1}{2}$.	True, $\Delta \log P$.	Approximate, $=5k\alpha t + \frac{1}{2}$.
Years.							
15-25	·0297	·0297	·0325	·0326	·0354	·0351	·0351
25-35	·0402	·0401	·0437	·0423	·0423	·0472	·0471
35-45	·0568	·0567	·0563	·0547	·0546	·0633	·0633
45-55	·0855	·0852	·0831	·0778	·0776	·0850	·0850
55-65	·1406	·1400	·1430	·1387	·1366	·1476	·1468
65-75	·2666	·2644	·2987	·2951	·2907	·3177	·3139
75-85	·6548	·6430	·6523	·6400	·6567	·6839	·6659
25-45	·0970	·0968	·1000	·0970	·0969	·1105	·1104
45-65	·2261	·2252	·2261	·2165	·2142	·2326	·2318
65-85	·9214	·9074	·9510	·9351	·9474	·1·0016	·9798
25-85	1·2445	1·2294	1·2771	1·2486	1·2585	1·3447	1·3220

Note.—The multiplier 5 in the approximate value of $\Delta \log_{10} P$ has been rendered necessary through $\alpha_{t+\frac{1}{2}}$ being used to signify the fifth part of the hyperbolic logarithm of the quinquennial rate of decrement at age $(t + \frac{1}{2})$.

TABLE VIII.—Annual Rates of Mortality per cent. in decennial intervals of age, observed in the Male Populations of Sweden and England at various periods, compared with the annual rates per cent. at the same ages given by the New and the Old Formula.

Interval of age.	Sweden and Finland.		Sweden.	England.		Theoretical Tables.	
	21 years, ending 1775.	20 years, ending 1795.	20 years ending 1840.	18 years, ending 1830.	7 years, ending 1844.	New formula.	Old formula.
Years.							
0-5	9·50	8·97	6·89	4·90	7·07	6·45	6·70
5-10	1·47	1·43	·78	·66	·93	·85	·99
10-20	·74	·68	·49	·55	·60	·60	·70
20-30	1·00	·98	·87	·93	·95	·79	·93
30-40	1·25	1·21	1·35	1·05	1·10	1·08	1·25
40-50	1·89	1·75	1·97	1·37	1·45	1·57	1·68
50-60	2·97	2·66	3·05	2·14	2·27	2·45	2·40
60-70	5·22	5·28	5·47	4·15	4·29	4·26	4·83
70-80	10·44	10·63	12·09	9·28	9·26	8·80	10·04
80-90	21·00	20·37	26·31	20·82	20·19	25·50	20·18
above 90	40·12	34·41	43·26	33·93	36·88	39·85
All ages	3·01	2·81	2·47	1·99	2·28		

II. *On the Bands formed by the Superposition of Paragenic Spectra produced by the Grooved Surfaces of Glass and Steel*.*—Part I.
By Sir DAVID BREWSTER, K.H., F.R.S.L. & E.†

[With a Plate.]

IN examining the colours produced by thin laminae of the crystalline lens of fishes, I observed a series of rectilineal serrated fringes perpendicular to the direction of the fibres, and produced by inclining the laminae in a plane cutting these fibres at right angles. I was thus led to imitate these fringes or bands by combining grooves or striæ cut upon glass or steel surfaces, or grooves taken from these surfaces upon isinglass or gums.

In my first experiments I combined a system of grooves on glass, executed for me by Mr. Dollond, with a similar system on steel executed by Sir John Barton, both of them containing 2000 divisions in an inch. The plate of glass was placed above the plate of steel, and slightly inclined to it, as shown in Plate I. figs. 1 and 2. The glass plate $A B C D$, fig. 2, was covered with grooves, but the steel plate below it was grooved only on the shaded portion $a b c d$, the parts $A a c C$, $B b d D$ being polished so as to reflect to the eye at E (fig. 1), the grooves on the glass when illuminated by rays, $R r$, proceeding from the first pair of the paragenic spectra produced by the grooves.

When the direction of the grooves $a c$ is nearly parallel to the plane of reflexion, and to one another, a series of minute serrated bands is seen on the space $a b c d$, where the light has been transmitted twice through the grooves on glass, and reflected once from those on steel; but no bands are seen upon $A a c C$, $B b d D$, where the steel was only polished.

When the grooves were slightly inclined to the plane of reflexion, large serrated bands appeared upon the spaces $A a c C$, $B b d D$; and when this inclination was increased, these large bands became smaller and more numerous, crowding towards $C c$ and $d D$. On the other hand, they become larger and larger as the direction of the grooves returned into the plane of reflexion. In the azimuth of 0° they become straight, and by increasing the azimuth, they pass, as it were, to the right hand, as shown in fig. 3.

When the direction of the grooves is inclined to the plane of

* In a very interesting paper on the Spectra produced by Gratings or Grooved Surfaces, M. Babinet has given them the appropriate name of *paragenic*, in order to distinguish the spectra produced by refraction from those produced by the lateral propagation of light. “*Sur la Paragénie ou propagation latérale de la lumière.*” Paris, 1864. Extrait du *Cosmos*.

† From the Transactions of the Royal Society of Edinburgh, vol. xxiv. part 1. Communicated by the Author.

reflexion, the minute serrated bands upon *a b c d* become smaller and less serrated.

When the inclination *mn* *N M* of the grooved plates is increased, the large bands become smaller and smaller; and when it is diminished, they become larger and larger, getting inclined as in fig. 3, and becoming parallel at 0° of inclination.

Having been provided, by the kindness of Sir John Barton, with two grooved plates of glass containing 500 divisions in an inch, I was enabled to examine the fringes on the paragenic spectra under different circumstances.

When the grooved surfaces of the plates were placed in contact, and the grooves formed a small angle with one another, the middle or principal image, *A* (fig. 4), when observed with a lens whose anterior focus coincided with the grooves, had no bands, but the paragenic spectra *a, c, b, d* on each side had numerous serrated bands or fringes perpendicular to the direction of the grooves, the number on the first spectra *a, b* being at the rate of 19 in an inch of the luminous disk, and increasing in arithmetical progression.

When the luminous object is rectangular, and the rectangular paragenic spectra are brought nearly into contact, as at *a b* and *c d* (fig. 5), the bands, as seen at nearly a perpendicular incidence, are shown in this figure.

When the incident light is inclined to the direction of the grooves, the bands suffer no change, and appear immoveable on the surface of the glass plates.

When the ray of light is perpendicular to the direction of the grooves, and the surface of the glass on which they are cut is inclined to the ray of light, the bands all descend from *a* to *b* (fig. 5), moving off, as it were, at *b* and *d* and succeeded by others when the angle of incidence increases, while they ascend from *b* to *a* and from *d* to *c*, moving off at *a* and *c*, when the angle of incidence diminishes. In this case the grooves of the plate next the eye are turned to the left, the opposite motions taking place when they are turned to the right*.

The bands correspond to the intersection of the one set of grooves with the other set; and consequently they diminish in number and recede from one another when the inclination of the one set of grooves to the other diminishes, becoming parallel to the grooves when the grooves on both plates are parallel.

Interference bands parallel to the grooves may be seen by transmitted light upon the paragenic spectra when two systems of grooves are placed parallel to each other, and when the grooves in the one system are parallel to those in the other.

* This motion of the bands is not seen when the grooved surfaces are perfectly parallel.

They are seen both at a perpendicular incidence and when the plates are inclined in a plane parallel to the grooves.

These bands become narrow as the distance of the two grooved surfaces is increased, and they are seen at all angles of incidence, and in all planes of reflexion from the grooved surfaces.

I have observed these bands, which are generally more or less serrated, in combinations of 1000 with 1000, 1000 with 2000, 1000 with 500, 2000 with 500, and in the combination of four surfaces of 2000, 1000, 100, and 500.

In the combination of 1000 and 500, and in no other, a very peculiar system of bands is seen with a lens. They are not serrated, and not perpendicular to the grooves. The system consists of two sets equally inclined to the direction of the grooves, when the grooves in one plate are slightly inclined to those in the other. By diminishing the inclination of the grooves, the inclination of the bands to the direction of the grooves diminishes, and when the grooves become parallel, the bands become parallel and disappear.

These bands must have a different origin from those previously described, as they are similar in number upon all the prismatic images.

In these experiments the duplication of the bands on the second spectrum, and their increase in arithmetical progression on the other spectra, is a remarkable fact which it is difficult to explain. The second spectrum differs from the first, and the third from the second, only in their length; and we can hardly suppose that they have a property in a direction perpendicular to their length, or to Fraunhofer's lines, which would increase the number of their bands.

The bands which we have described are more distinct when the spectra are pure or formed from a narrow line or bar of light; but when we wish to see the bands on the bar of light or the central image A (fig. 4), the spectra must be formed from wide spaces, which gave impure spectra.

In order to examine the interference bands under different conditions, I placed (as in fig. 6) a plate of polished steel at different distances from another plate of steel containing six systems of grooves executed by Sir John Barton, varying from 312.5 divisions in an inch to 10,000. When the light was reflected twice from the grooved surface and once from the plain steel surface, the bands which covered the colourless image and the paragenic spectra were splendid beyond description, and unlike anything of the kind that I had previously seen.

1. The bands were parallel to the grooves, or to the lines in the spectra.

2. They are smaller and more numerous when the grooves are wider or fewer in an inch.

3. They become smaller and more numerous when the distance of the plates is increased.

4. They are smaller and more numerous when the angle of incidence is increased.

5. They become more numerous by increasing the number of reflexions.

6. They appear like minute black lines upon the colourless image, but when their magnitude is increased, they appear like blue or pink bands on a ground of a different colour, which is generally white or whitish blue.

These bands were visible on the systems of grooves 312·5, 625, 1250, and 2500 in an inch, but not on the systems of 5000 or 10,000 in an inch.

When the spectra had suffered three, four, five, or six reflexions, the central and other images were covered with the same number of bands as with two reflexions from the grooved steel; but another series of wider bands was superposed.

The following results were obtained with grooved surfaces having 1250 divisions in an inch:—

Distance of plates.	0·11 in.	Angular breadth of each ..	7° 50'
Distance of circular disk	115·5 „	Distance of plates	0·22 in.
Diameter of disk	1·317 „	Angle of incidence	63° 30'
Angle of incidence	63° 30'	Number of fringes on the disk {	10
Angular diameter of disk.	39° 30'	and on the first spectrum. {	
Number of fringes on disk {	5	Angular breadth of each ..	3° 55'
and on the first spectrum {			

In order to observe the effect produced by varying the angle of incidence, I placed a luminous disk $3\frac{6}{10}$ inches in diameter* at the distance of 9 feet 6 inches from the grating, and obtained the following results:—

Angle of incidence.	No. of bands on the disk.	Angle of incidence.	No. of bands on the disk.
70°	29	50°	17
60°	21	40°	14

The bands were seen at an incidence of $87\frac{1}{4}^\circ$ when the plates were nearly in contact.

The following were the colours seen on the two spectra on one side of the colourless image; but I have not measured the precise angle of incidence at which they were seen, nor mentioned in my journal whether they were seen with the 625 or the 1250 grating:—

* This disk included part of the spectrum on each side of the bright image.

First spectrum.		Second spectrum.	
Great incidences.	White.	Great incidences.	Blue.
	Pale red.		Bluish.
	Red.		Less blue.
	Purple.		Bluish white.
	Blue.		White.
	Bluish.		Pale red.
	Less blue.		Red.
Lesser incidences.	White.	Lesser incidences.	{ Purple.
			{ Blue,

At small angles of incidence, about 42° , the bands become less distinct and paler in colour, the white becoming yellow, and the blue brownish.

In the systems of grooves, whether on glass or on steel, employed in the preceding experiments, the part of the original surface not removed by the grooves bears a very considerable proportion to the part removed; but when the grooves occupy a large part of the surface, and the intermediate parts a very small one, a new set of phenomena are produced, which must change in a remarkable manner all the bands of interference. The execution, however, of such systems of grooves is very difficult. Sir John Barton, with all his experience, failed in producing good specimens; but even with those which he executed for me, phenomena of a remarkable kind were exhibited not only on the middle or colourless image, but upon all the paragenic spectra, varying with the number of grooves, but still more remarkably with the angle of incidence*.

P.S.—The preceding experiments were made in 1823 and 1827, and those described in p. 24 were repeated in 1838. Having lost or mislaid the glass gratings which I then employed, I am not able to compare the bands which they produced with a more remarkable series which I have recently obtained with new gratings, and which will be the subject of another communication.

III. *On the Excentricity of the Earth's Orbit.*

By JAMES CROLL†.

THE following Table contains the values of the excentricity of the earth's orbit and longitude of the perihelion for a million of years past and a million of years to come. They have been calculated for the purpose of arriving at some better knowledge regarding the general character of those secular changes

* See Phil. Trans. 1829, p. 301.

† Communicated by the Author.

of climate which have been proved to result from excentricity. The values have been determined at epochs 50,000 years apart. From the extreme slowness with which the excentricity changes, it was not deemed necessary for our purpose to determine them at shorter intervals. The longitude of the perihelion is given merely to show the great irregularity of the motion of the major axis.

We have already stated it as our opinion that the glacial epoch of the geologist was the period beginning about 240,000 years ago and extending down till about 80,000 years ago ('Reader,' October 14, December 2 and 9). The time of the greatest cold would be about 200,000 or 210,000 years ago.

It will be seen from an inspection of the Table that the next glacial epoch prior to this occurred about 750,000 years ago. At that time the excentricity was exactly equal to what it was 210,000 years ago. Going back 50,000 years further, we find the excentricity to be only 0.0132. But 50,000 years still further back, viz. 850,000 years ago, the excentricity almost reached its superior limit. It is quite possible that this, and not 200,000 years ago, may have been the period of the boulder-clay. Proceeding backwards the excentricity is again found to diminish, but at the period 950,000 years ago it reached the high value of 0.0517. Here we have three glacial epochs following each other in close succession—or rather, we should say, one long glacial epoch of about 250,000 years broken up by two mild periods 100,000 years apart.

It is scarcely necessary to remind the reader that the mild periods to which we allude have no resemblance to what we have designated the warm periods of the glacial epoch resulting from the occurrence of the winter solstice in the perihelion. During those mild periods which occurred 800,000 and 900,000 years ago, when the excentricity was at a low value, the general character of the earth's climate would be similar to what it is at present; but during the warm, or rather, we should say, equable periods resulting from the position of the solstice-point in relation to the perihelion, the climate would be widely different; for during these periods the winters would be about as warm as the summers. (See 'Reader,' December 9.)

From the Table it will be seen that a similar condition of things will occur between 800,000 and 1,000,000 years to come. There will occur three glacial epochs in succession—namely, at the periods 800,000, 900,000, and 1,000,000 years to come.

The calculations have been made from formulæ given by M. Leverrier in his "Memoir on the Secular Variations of the Elements of the Orbits of the Planets," published in the "Additions" to the *Connaissance des Temps* for 1843.

Past time.			Future time.		
Number of years before epoch 1800.	Excentricity.	Longitude of perihelion.	Number of years after epoch 1800.	Excentricity.	Longitude of perihelion.
1,000,000	0-0151	248° 22'	50,000	0-0173	38° 12'
950,000	0-0517	97 51	100,000	0-0191	114 50
900,000	0 0102	135 2	150,000	0-0353	201 57
850,000	0-0747	239 28	200,000	0-0076	303 30
800,000	0-0132	343 49	250,000	0-0286	350 54
750,000	0-0575	27 18	300,000	0-0158	172 29
700,000	0-0220	208 13	350,000	0-0098	201 40
650,000	0-0226	141 29	400,000	0-0429	6 9
600,000	0-0417	32 34	450,000	0-0231	98 37
550,000	0-0166	251 50	500,000	0-0534	157 26
500,000	0-0388	193 56	550,000	0-0259	287 31
450,000	0 0308	356 52	600,000	0-0395	285 43
400,000	0-0170	290 7	650,000	0-0169	144 3
350,000	0-0195	182 50	700,000	0-0357	17 12
300,000	0-0424	23 29	750,000	0-0195	0 53
250,000	0-0258	59 39	800,000	0-0639	140 38
200,000	0-0569	168 18	850,000	0-0144	176 41
150,000	0-0332	242 56	900,000	0-0659	291 16
100,000	0-0473	316 2	950,000	0-0086	115 13
50,000	0-0131	50 3	1,000,000	0-0528	57 31
0	0-0168	99 30			

IV. On the determination of the Disgregation of a Body, and on the True Capacity for Heat. By Professor CLAUSIUS*.

IN my memoir "On the Application of the principle of the Equivalence of Transformations to Internal Work"†, I introduced into the theory of heat a new quantity having reference to the arrangement of the particles of a body, and which I called the *disgregation* of the body. This quantity serves to express the total quantity of work which heat can do when the particles of the body undergo changes of arrangement at different temperatures. Let us suppose that the body undergoes an infinitely small change of state, the change being such that it is reversible, and let the entire work done during this change be represented by dL ; further, let the absolute temperature of the body be called T , and let the calorimetric equivalent of the unit of work be A . We shall then have, as I have shown in the

* Communicated by the Author, from the *Archives des Sciences Physiques et Mathématiques* for October 1865, having been read August 22, 1865, before the Société helvétique des Sciences naturelles à Genève.

† Poggendorff's *Annalen*, vol. cxvi. p. 73; Liouville's *Journal*, 2nd series, vol. vii. p. 209. [Phil. Mag. S. 4. vol. xxiv. p. 81.]

paper referred to, the following equation,

$$dL = \frac{T}{A} dZ, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where Z is a quantity completely determined by the actual condition of the body, and independent of the way in which the body arrived at that condition. If the state of the body is determined by two variable quantities, Z will be a function of these variables. And it is this quantity Z which I have called the *disgregation* of the body.

The total quantity of work L , an element of which occurs in equation (1), is made up of the internal work and the external work, which I will denote respectively by J and W . The internal work J is a quantity which can be expressed, like the disgregation, by a function of the two variables which determine the actual condition of the body. The external work W , on the contrary, depends not only on the actual condition of the body, but also upon the way in which it came into that condition.

If we suppose that the temperature T and the volume v are the two variables which determine the condition of the body, we may write

$$dZ = \frac{dZ}{dT} dT + \frac{dZ}{dv} dv,$$

$$dJ = \frac{dJ}{dT} dT + \frac{dJ}{dv} dv.$$

For the external work W , in case the only external force which has to be overcome during the change of condition is a pressure p acting on the surface of the body, we have the equation

$$dW = p dv.$$

By introducing these values of dZ , dJ , and dW into equation (1), after substituting $dJ + dW$ for dL , we obtain

$$\frac{dJ}{dT} dT + \left(\frac{dJ}{dv} + p \right) dv = \frac{T}{A} \left(\frac{dZ}{dT} dT + \frac{dZ}{dv} dv \right),$$

and hence we get

$$\left. \begin{aligned} \frac{T}{A} \cdot \frac{dZ}{dT} &= \frac{dJ}{dT}, \\ \frac{T}{A} \cdot \frac{dZ}{dv} &= \frac{dJ}{dv} + p. \end{aligned} \right\} . \quad . \quad . \quad . \quad . \quad (2)$$

From these equations a very simple value of the differential $\frac{dZ}{dv}$ can be deduced. For this purpose we must differentiate the

first equation with respect to v , and the second with respect to T ; we then obtain

$$\frac{T}{A} \cdot \frac{d^2 Z}{dT dv} = \frac{d^2 J}{dT dv},$$

$$\frac{1}{A} \cdot \frac{dZ}{dv} + \frac{T}{A} \cdot \frac{d^2 Z}{dT dv} = \frac{d^2 J}{dT dv} + \frac{dp}{dT}.$$

By subtracting the first of these equations from the second, and multiplying the remainder by A , we get the expression sought, namely,

$$\frac{dZ}{dv} = A \frac{dp}{dT}. \quad (3)$$

If we combine this expression with the expression for $\frac{dZ}{dT}$ which results from the first of the equations (2), we can form the complete differential equation which follows,

$$dZ = \frac{A}{T} \cdot \frac{dJ}{dT} dT + A \frac{dp}{dT} dv. \quad (4)$$

In order to integrate this equation, we will take as our starting-point a condition in which the temperature and volume are T_0 and v_0 , and will denote the corresponding value of Z by Z_0 . Let us now suppose that, in the first place, the temperature varies from T_0 to any other value T , the volume remaining unchanged at v_0 —and that, in the second place, the volume varies from v_0 to v , while the temperature remains constant at T ; we shall then, by following in our integration the order of changes of state here denoted, obtain the equation

$$Z = Z_0 + A \int_{T_0}^T \left(\frac{1}{T} \cdot \frac{dJ}{dT} \right)_{v=v_0} dT + A \int_{v_0}^v \frac{dp}{dT} dv. \quad (5)$$

In my paper I have compared the quantity Z , determined in the manner that has been explained, with a quantity which Professor Rankine has denoted by F , and which is defined by the equation

$$F = \int \frac{dp}{dT} dv, \quad (6)$$

where the integration ought to be taken from a given initial volume to the actual volume, the temperature being supposed to remain constant. I have stated that this quantity F is not identical with the quantity $\frac{1}{A} Z$, but that it differs from it in general by a function of T . It is easy to see that the function

of T in question is the integral

$$\int_{T_0}^T \left(\frac{1}{T} \cdot \frac{dJ}{dT} \right)_{v=v_0} dT,$$

which enters into the expression for $\frac{1}{A} Z$ given by equation (5), but does not occur in the expression for F . I have also stated that in the case where $\frac{dJ}{dT} = 0$, as happens for the perfect gases, these two quantities may be regarded as equal.

In an exposition of the mechanical theory of heat recently published by M. Paul de Saint-Robert*, this talented author expresses the opinion that the difference insisted on by me between the quantities F and $\frac{1}{A} Z$ does not exist. But I cannot agree with his reasoning, and in my opinion the simplifications which he has introduced into the formulæ, by means of this reasoning, are not generally admissible.

M. de Saint-Robert supposes that if the free space afforded to the body is very large, the body will be reduced at all temperatures to the state of a perfect gas—that is to say, to a state in which there is no internal work, and in which consequently we have $\frac{dJ}{dT} = 0$. Under these circumstances, if, in equation (5), we take as the initial state one in which the volume v_0 is very great, we shall have

$$\int_{T_0}^T \left(\frac{1}{T} \cdot \frac{dJ}{dT} \right)_{v=v_0} dT = 0, \quad (7)$$

and consequently equation (5) is reduced to

$$Z = Z_0 + A \int_{v_0}^v \frac{dp}{dT} dv. \quad (8)$$

We thus arrive at the result that the quantity $\frac{1}{A} Z$ is identical with the quantity F as defined by equation (6).

But it will be seen that the accuracy of this conclusion depends on the accuracy of M. de Saint-Robert's supposition. This, therefore, is the point which specially demands our attention.

M. de Saint-Robert says, at the end of his reflections on this subject (p. 91 of his book), that he supposes that all natural bodies can be caused by heat to pass into the state of perfect

* *Principes de Thermodynamique*, par Paul de Saint-Robert. Turin, 1865.

gases; and he adds, "Although there exist bodies which do not yield to the means at our disposal, we are nevertheless justified in concluding, from all known experiments, that all bodies converge, in proportion as their temperature is raised, towards this condition of perfect gas; and this is enough for our argument."

But this passage does not correspond with his calculations. In order that equation (7), by means of which equation (5) is reduced to equation (8), may hold as a general expression, we require to have

$$\frac{dJ}{dT} = 0$$

not only at very high temperatures, but at all temperatures under consideration. We must therefore, if we admit M. de Saint-Robert's formulæ, suppose that every substance passes *at all temperatures* into the condition of a perfect gas when the space afforded to it for expansion is sufficiently great.

But there are many bodies for which it seems to me that this does not hold good; it could not be said, for instance, of a piece of iron, quartz, or any other similar substance, that it is sufficient to increase the space into which it can freely expand in order to cause it to pass at low temperatures into the condition of a perfect gas.

Even such substances as water, carbonic acid, and other compound liquids or gases, present greater difficulties than might perhaps be supposed at a first glance. We know, more particularly by the beautiful experiments of M. H. Sainte-Claire Deville, that these bodies can undergo dissociation by the action of heat. This dissociation no doubt involves internal work. Unless, therefore, we suppose that complete dissociation occurs at all temperatures, when the volumes are very great, we cannot assume that the equation

$$\frac{dJ}{dT} = 0$$

is true at all temperatures.

We see from this that the expression for $\frac{1}{A} Z$ derived from equation (5) is not generally identical with the expression F given by equation (6), but that it is only in particular cases that these two quantities can be regarded as equal, which is just what I said at the outset.

I will venture to add, in conclusion, a few words upon another subject.

There is an essential difference between my views and those

of Professor Rankine as to the true capacity of bodies for heat. Professor Rankine considers that the true capacity for heat of the same body can have different values when the states of aggregation of the body are different; whereas I, on the other hand, have given my reasons for supposing that the true capacity of a body for heat must be the same in all states of aggregation*.

M. de Saint-Robert now makes this same supposition, that the capacity of a body for heat is the same under all states, and consequently that the quantity of heat which a body contains is proportional to its absolute temperature; but instead of referring to the reasons which had led me to this conclusion, he merely says (page 83), "The temperature t being the outward manifestation of the quantity of heat H contained in a body under its original form of heat, it follows that whenever a body has the same temperature, it must have the same quantity of internal heat."

I cannot think that this argument will be regarded as conclusive. It does not appear to me directly evident that the outward manifestation of the heat must be the same in the different states of aggregation. If the conclusion in question could be deduced in so simple a manner, so quick-sighted a philosopher as Dr. Rankine would assuredly not maintain the opposite opinion.

V. *Supplementary Researches in Hydrodynamics*.—Part III.

By Professor CHALLIS, M.A., F.R.S., F.R.A.S.†

THE Hydrodynamical Researches communicated in the Numbers of the Philosophical Magazine for September and October 1865, which were mainly devoted to the consideration of the problem of the motions of a small sphere acted upon by the undulations of an elastic fluid, carried the solution of it so far as to evolve expressions for the acceleration of the sphere containing two undetermined arbitrary constants m_1 and m'_1 . It will be my endeavour, in continuing the Researches, to complete the solution by ascertaining the composition and values of these quantities. Having found, in the course of revising for this purpose the reasoning in the previous researches, that it requires some modifications, I shall commence with pointing these out. The novelty and the difficulty of the mathematical investigations involved in the treatment of this problem may be alleged as sufficiently explaining why I am obliged to proceed by slow and tentative steps. My reasons for considering the solution of

* [Professor Rankine's remarks on these observations will be found at p. 407 of the preceding Number.—ED.]

† Communicated by the Author.

Phil. Mag. S. 4. Vol. 31. No. 206. Jan. 1866.

it to be absolutely necessary for the advancement of physical theory have been fully stated in previous communications.

For the better understanding of the present argument it will be proper, in the first instance, to advert briefly to several previously established theorems. One of these theorems is, that the third general equation conducts to rectilinear motion by merely supposing that $udx + vdy + wdz$ is an exact differential. As this is an abstract analytical supposition, made without reference to any case of arbitrary disturbance, we may infer from the result that rectilinearity is a general characteristic of the motion of a fluid, in so far as the motion is not determined by arbitrary conditions. To ascertain whether the straight line along which the motion takes place is an *axis* relative to the contiguous motion, I have supposed that $(d.f\phi) = udx + vdy + wdz$, and that ϕ is a function of z and t , and f a function of x and y , such that $f=1$, $\frac{df}{dx}=0$, and $\frac{df}{dy}=0$, where $x=0$, and $y=0$. This supposition is justified by finding that, on introducing it into the two other general equations, an equation is obtained from them which, for points contiguous to the axis, is resolvable into the two following:—

$$\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} + \frac{b^2f}{a^2} = 0,$$

$$b^2\phi - a^2 \frac{d^2\phi}{dz^2} + \frac{d^2\phi}{dt^2} + 2 \frac{d\phi}{dz} \cdot \frac{d^2\phi}{dzdt} + \frac{d\phi^2}{dz^2} \cdot \frac{d^2\phi}{dz^2} = 0.$$

Of each of these equations I have obtained a *particular* solution—that is, a solution of explicit form to which the analysis conducts without reference to arbitrary conditions of the motion. (See an Article in the Philosophical Magazine for December 1852.) By the process referred to, the first equation gives

$$f = 1 - er^2 + \frac{e^2r^4}{1^2 \cdot 2^2} - \frac{e^3r^6}{1^2 \cdot 2^2 \cdot 3^2} + \&c.,$$

r being the distance from the axis of z , which is the axis of motion, and e being put for $\frac{b^2}{4a^2}$; and the other equation gives for $\frac{d\phi}{dz}$, or the velocity (w') along the axis,

$$w' = m \sin q\mu - \frac{2m^2\kappa}{3a(\kappa^2 - 1)} \cos 2q\mu + \&c.,$$

q being put for $\frac{2\pi}{\lambda}$, μ for $z - a_1 t + c$, and κ being the ratio of a_1 , the velocity of propagation, to a . Also by means of the equation

$$a^2 \text{Nap. log } \rho + f \frac{d\phi}{dt} + \frac{f^2}{2} \cdot \frac{d\phi^2}{dz^2} = 0,$$

is obtained, for the value of the condensation (σ') along the axis,

$$\sigma' = \frac{\kappa m}{a} \sin q\mu - \frac{2m^2\kappa^2}{3a^2(\kappa^2-1)} \cos 2q\mu + \frac{(\kappa^2-1)m^2}{2a^2} \cdot \sin^2 q\mu + \&c.$$

Now, since $udx + vdy + wdz$ is strictly an exact differential only for indefinitely small distances from the axis, the above value of f is strictly true only for indefinitely small values of r ; so that for the purposes of exact calculation $f=1-er^2$. On the contrary, the exact values of w' and σ' are expressed by series consisting of an unlimited number of terms, each of which is a circular function of $z - a_1 t + c$. These expressions accordingly prove that w' and σ' are propagated along the axis with the same constant velocity a_1 , and without undergoing alteration. The proof of this law is independent of the value of m ; and whether w' and σ' be large or small, the above series retain the same form, the terms following the first or principal terms always coexisting with the latter. For our present purpose it suffices to include only terms of the second order with respect to m , in which case κ is a numerical constant, the analytical expression for which is $(1 + \frac{e\lambda^2}{\pi^2})^{\frac{1}{2}}$.

Again, it is to be remarked that since explicit formulæ expressing the velocity and condensation in vibratory motion have been obtained without reference to a given mode of disturbance, in every case of disturbance producing vibrations of the fluid the motion must in a certain manner be compounded of the motions defined by these formulæ. It is therefore requisite to inquire, next, respecting the laws and the effects of this composition.

From the usual approximate equations

$$\frac{a^2 \cdot d\sigma}{dx} + \frac{du}{dt} = 0, \quad \frac{a^2 \cdot d\sigma}{dy} + \frac{dv}{dt} = 0, \quad \frac{a^2 \cdot d\sigma}{dz} + \frac{dw}{dt} = 0,$$

the known theorem that $udx + vdy + wdz$ is approximately an exact differential for small vibratory motions may be deduced prior to the consideration of any arbitrary disturbance. Let us suppose that in this case also that differential is equal to $(d \cdot f\phi)$, f and ϕ being functions of the same variables as before. Then from the above equations, and from the approximate equation of constancy of mass, viz.

$$\frac{d\sigma}{dt} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0,$$

there results,

$$\phi \left(\frac{d^2 f}{dx^2} + \frac{d^2 f}{dy^2} \right) + f \frac{d^2 \phi}{dz^2} - \frac{f}{a^2} \cdot \frac{d^2 \phi}{dt^2} = 0.$$

The assumed compositions of f and ϕ are satisfied by resolving

this equation into the two following,

$$\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} + \frac{b^2f}{a^2} = 0,$$

$$b^2\phi - a^2 \cdot \frac{d^2\phi}{dz^2} + \frac{d^2\phi}{dt^2} = 0,$$

which are both linear with constant coefficients. It is evident that, both in this and in the former resolution, $-\frac{b^2}{a^2}$ is the value of $\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2}$, where $x=0$ and $y=0$. The particular solution of the first of these equations is the series for f already given, in which the value of r may now be taken without limitation. From the particular solution of the other equation, combined with the equations

$$\omega = f \frac{d\phi}{dz}, \quad a^2\sigma + f \frac{d\phi}{dt} = 0,$$

we obtain

$$\omega = \frac{a\sigma}{\kappa} = fm \sin \frac{2\pi}{\lambda} (z - \kappa at + c).$$

Also the transverse velocity

$$= \phi \frac{df}{dr} = -\frac{m\lambda}{2\pi} \cdot \frac{df}{dr} \cdot \cos \frac{2\pi}{\lambda} (z - \kappa at + c).$$

The definite expressions thus arrived at apply to the *principal* parts of the condensation, and of the velocities parallel and transverse to the axis—that is, to the parts which are of the same order as the velocity and condensation indicated by the first terms of the series for w' and σ' . It has already been argued that the parts of the velocity and condensation along the axis indicated by the other terms necessarily coexist with those that are principal; but with respect to points distant from the axis, expressions for the velocity and condensation to terms of a higher order than the first are not deducible by the preceding investigation, because the assumptions made respecting the properties of the functions f and ϕ are not satisfied beyond terms of the first order. It may, however, be assumed that the motion and condensation are symmetrically disposed about the axis, this having been shown to be exactly the case in its immediate neighbourhood, and very nearly so at all other positions. In fact, by supposing that $(d\psi) = (d \cdot f\phi + d\chi) = udx + vdy + wdz$, χ being a function of z , r , and t , and by substituting in the general differential equation to terms of the second order, of which ψ is the principal variable, viz.

$$0 = a^2 \left(\frac{d^2 \psi}{dx^2} + \frac{d^2 \psi}{dy^2} + \frac{d^2 \psi}{dz^2} \right) - \frac{d^2 \psi}{dt^2} \\ - 2 \frac{d\psi}{dx} \cdot \frac{d^2 \psi}{dx dt} - 2 \frac{d\psi}{dy} \cdot \frac{d^2 \psi}{dy dt} - 2 \frac{d\psi}{dz} \cdot \frac{d^2 \psi}{dz dt},$$

I have found that the equation is satisfied if

$$\chi = m^2 f_1 \sin 2q (z - \kappa at + c),$$

f_1 being determined by the following equation,

$$\frac{d^2 f_1}{dr^2} + \frac{df_1}{r dr} + 16ef_1 = \frac{\kappa}{qa} \left(\frac{df^3}{dr^2} - q^2 f^2 \right).$$

The value of f expressed in a series being known, a series for f_1 may be readily deduced from this equation by the method of indeterminate coefficients. In this way I have obtained

$$f_1 = -\frac{\kappa q r^2}{4a} \cdot \left\{ 1 - \frac{\kappa^2 + 5}{4} e r^2 + \frac{4\kappa^2 + 11}{18} e^2 r^4 - \&c. \right\}$$

There is another consideration which it will be proper to introduce in this stage of the argument. Since the differential equations which determine f and ϕ to quantities of the first order are linear with constant coefficients, it might be supposed that we may hence infer the coexistence of small vibrations.

But because the value of the constant b^2 is $\frac{4\pi^2 a^2}{\lambda^2} (\kappa^2 - 1)$, it follows that the equation

$$b^2 \phi - a^2 \frac{d^2 \phi}{dz^2} + \frac{d^2 \phi}{dt^2} = 0$$

is satisfied by only a single value of λ , and that for every different value the equation is different. In consequence of this analytical circumstance, no general inference respecting the coexistence of small vibrations can be drawn from the above equation. In fact the foregoing investigations are only proper for determining *forms* of vibratory motion that are independent of arbitrary conditions. It is true that the equation might be satisfied by supposing ϕ to be the sum of any number of terms such as $m \sin \frac{2\pi}{\lambda} (z - \kappa at + c)$, λ being the same for all; but in that case,

as is known, the form of the sum would be the same as that of each term. It may, however, be said that in virtue of this composition the factor m might be assumed to be a particular constant, in which case the difference as to magnitude between one set of vibrations and another having the same value of λ would depend exclusively on the *number* of the components and their respective phases. Having premised so much as this, I proceed now to show that the velocities and condensations relative to dif-

ferent axes may coexist even when all the terms of the series for w' and σ' are taken into account.

It is not necessary to give in detail the well-known proof of the integrability of $udx + vdy + wdz$ for small vibratory motions. The proof rests exclusively on the suppositions that the velocity is very small compared to a , and that no part of it is independent of the time. Now these two conditions are satisfied by the series for w' , if only m be very small compared to a , because in that case the first term is much larger than the sum of all the others. They are also satisfied by the motion at any distance from the axis, since it consists of vibrations, and is of less magnitude than that along the axis. The foregoing determination of the function χ , after supposing $(d \cdot f\phi + d\chi)$ to be equal to $udx + vdy + wdz$, is a direct proof, to terms of the second order, of the integrability of that differential for positions at any distance from the axis. If we suppose generally that

$$(d\psi) = udx + vdy + wdz,$$

we shall obtain, to small quantities of the first order, the known linear equation

$$\frac{d^2\psi}{dt^2} = a^2 \left(\frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} + \frac{d^2\psi}{dz^2} \right),$$

which, it should be noticed, does not contain any constant of specific value, such as b^2 , nor involve any assumption respecting the composition of ψ . Now the motion relative to an axis, while obeying the laws above determined, must at the same time satisfy this equation, whether or not terms involving m^2 , m^3 , &c. be included, solely for the reasons that the whole motion is, by hypothesis, very small compared to a , and, being vibratory, is such as to make $udx + vdy + wdz$ integrable. Hence, if ψ_1 represent the value of ψ for one such set of vibrations, we shall have

$$\frac{d^2\psi_1}{dt^2} = a^2 \left(\frac{d^2\psi_1}{dx^2} + \frac{d^2\psi_1}{dy^2} + \frac{d^2\psi_1}{dz^2} \right).$$

So for another,

$$\frac{d^2\psi_2}{dt^2} = a^2 \left(\frac{d^2\psi_2}{dx^2} + \frac{d^2\psi_2}{dy^2} + \frac{d^2\psi_2}{dz^2} \right).$$

$$\&c. = \&c.$$

Consequently if $\Phi = \psi_1 + \psi_2 + \psi_3 + \&c.$, the same equation will be satisfied by the value Φ of ψ , and we have also

$$\frac{d\Phi}{dx} = \frac{d\psi_1}{dx} + \frac{d\psi_2}{dx} + \&c., \quad \frac{d\Phi}{dy} = \frac{d\psi_1}{dy} + \frac{d\psi_2}{dy} + \&c.,$$

$$\frac{d\Phi}{dz} = \frac{d\psi_1}{dz} + \frac{d\psi_2}{dz} + \&c.$$

This reasoning proves that the different sets of vibrations may coexist, and that the resulting compound motion is the sum of the separate motions. It follows also that there may be an unlimited number of axes of motion having their positions in space perfectly arbitrary.

Again, from the approximate general equations already cited, it is readily shown that

$$\frac{d^2\sigma}{dt^2} = a^2 \cdot \left(\frac{d^2\sigma}{dx^2} + \frac{d^2\sigma}{dy^2} + \frac{d^2\sigma}{dz^2} \right).$$

To obtain this equation, no other condition is required than that the motion be small compared to a . It is not necessary to suppose either that $udx + vdy + wdz$ is an exact differential, or that σ contains no part which is independent of the time. If, however, the equation be applied to vibratory motion, the former of these conditions is, as we have seen, in fact satisfied; but no necessity exists for the fulfilment of the latter. Now the foregoing series for σ' , inasmuch as it contains terms involving the squares of the sines of circular arcs, has a value which is partly independent of the time. But for the reason just adduced, the above linear differential equation is nevertheless applicable to vibratory motion relative to an axis, and by means of it the coexistence of the condensations of different sets of vibrations may be proved by reasoning analogous to that by which the coexistence of the velocities was inferred. It is evident that these considerations were necessary to complete the proof of the coexistence of vibratory motions relative to axes; which, as obeying laws that are independent of arbitrary circumstances, may for distinction be called *free vibrations*.

Let us now make an application of the foregoing principles and results by taking the simple case of motion compelled by arbitrary conditions to be wholly in straight lines perpendicular to a plane. Since arbitrarily impressed motion must in any case be such as to result from the composition of free motions, it may, in this instance, be supposed to be compounded of an unlimited number of free motions having their axes all perpendicular to the plane, and distributed in such manner that the transverse motions are destroyed. In that case, if V_1 be the given arbitrary velocity, so impressed that the propagation of the motion is wholly in the positive direction, and if w be the velocity relative to any one axis, in the direction parallel to it, and at any distance from it, we shall have

$$V_1 = k \cdot [\Sigma \cdot w] = f(z - \kappa at + c),$$

the form of the function f being determined by the arbitrary disturbance, and $\Sigma \cdot w$ representing the sum, at a given position,

of the values of w for all the axes. V_1 is supposed to be proportional, but not equal, to this sum, for reasons which will be given presently. It is here assumed that any arbitrary function of $z - \kappa at + c$ may be expressed by the sum of an unlimited number of terms such as $m \sin \frac{2\pi}{\lambda} (z - \kappa at + c)$, m , λ , and c being at disposal. Again, if σ_1 be the composite condensation corresponding to V_1 , and σ the component condensation corresponding to w , by reason of the composition σ_1 will be a function of $z - \kappa at + c$, and the velocity of its propagation will be κa . But the form of the function will not be exactly the same as that of f , as will appear by comparing the two series for w' and σ' . Also, in consequence of the effect of the transverse motions, σ_1 will not be equal to $\Sigma \sigma$, but will be proportional to this sum. These assertions rest on the following considerations.

As by hypothesis the transverse motions are neutralized, and as each set of vibrations has been proved to be independent of the rest, it follows that the transverse motion relative to a given axis is just counteracted by the effect of all the other transverse motions. As far as regards the motion relative to that axis, the effect is the same as if the fluid were impressed with extraneous accelerative forces just equal and opposite to the transverse forces relative to the axes which are due to the action of the fluid. Now such impressed forces, being transverse, will not change the rate of propagation, but will alter the relation of V_1 to σ_1 . That this relation cannot be the same as that of w' to σ' will appear by considering that in the case of a single set of vibrations, the condensations along the axis are partly due to the longitudinal accelerations and partly to the transverse accelerations, whereas in the compound motion no part is due to transverse accelerations. We might consequently expect that the condensation corresponding to a given velocity would be less in the compound than in the simple motion. That this is actually the case may be proved as follows. Omitting in the series for w' and σ' the terms involving m^2 , m^3 , &c. as bearing insignificantly on this question, we find

that the velocity is equal to the product of $\frac{a}{\kappa}$ and the condensation; whilst in the case of the compound motion, the lines of motion being all parallel and the rate of propagation being κa , to the first order of small quantities the velocity, by a known theorem, is equal to the product of κa and the condensation. Thus the latter factor of the condensation is greater than the other in the proportion of κ^2 to 1, and consequently for a given velocity the condensation in the compound motion is in the same proportion less than the condensation in the simple motion.

If in the present instance of plane-undulations V_1 be the com-

pound velocity, and σ_1 the compound condensation, by the theorem just cited $V_1 = \kappa a \sigma_1$. It is here, however, to be remarked that since V_1 is wholly periodic, having as much positive as negative value, the same must be the case, according to this equality, with respect to the values of σ_1 . These values of σ_1 are therefore defective, not embracing the terms of $\Sigma \cdot \sigma$ which do not change sign. At the same time they include all those terms which have the same constant ratio to corresponding terms of $\Sigma \cdot w$, as, for instance, the first and second terms in the series for σ' . For the sake of distinctness, let us leave out of consideration terms involving higher powers of m than the second, the problem which is the ultimate object of these researches not requiring such to be taken into account, and let us suppose that $\sigma_1 + \Delta\sigma_1$ is the complete value of the compound condensation, $\Delta\sigma_1$ being proportional to the sum of the terms of $\Sigma \cdot \sigma$ which are of the form $A m^2 \sin^2 \frac{2\pi}{\lambda} (z - \kappa a t + c)$. We shall then have

$$V_1 = \kappa a \sigma_1 = f(z - \kappa a t + c_1).$$

Similarly, since different compound motions may coexist,

$$V_2 = -\kappa a \sigma_2 = F(z + \kappa a t + c_2).$$

the motion being impressed so that the propagation is in the negative direction. Hence if $V = V_1 + V_2$, and $\sigma = \sigma_1 + \sigma_2$ (the letter σ from this time receiving a new signification), we obtain from the above equations

$$\kappa^2 a^2 \cdot \frac{d\sigma}{dz} + \frac{dV}{dt} = 0.$$

This is the equation (γ) obtained in Part II. of the former Researches. It takes account of the composite character of the motion, and to the first order of approximation is of general application, as I have indicated in Part II. ; but it does not include the quantity $\Delta\sigma_1$. This part of the condensation, which, as I have before remarked, has the effect of making the excursions of the condensed particles equal to those of the rarefied particles, may be taken account of in a subsequent stage of the argument. It is in respect to the composition of this quantity that the present investigation is a modification of that which is given in Parts I. and II. In the October Number I have made use of the equation

$$\kappa a S = V + \frac{V^2}{\kappa a},$$

S and V representing the composite condensation and velocity, and have supposed that $\Delta\sigma_1$ is proportional to the second term of this equation. But the present argument has shown that it

is not allowable to take into account this second term, all the reasoning subsequent to the determination of the forms of the component vibrations having excluded terms of the order of the square of the velocity, whether the velocity be component or compounded. But it is legitimate to regard $\Delta\sigma_1$ as representing the part of the total condensation which corresponds to the *sum* of terms which contain m^2 as a factor and do not change sign, it having been proved that the law of coexistence of vibrations holds good notwithstanding that the expressions for the component condensations contain such terms.

This point being settled, I proceed to the determination of the constants m_1 and m'_1 . The mathematical reasoning which this investigation will require is simply additional to that contained in Part II. It will be supposed that the waves are incident on the sphere, regarded as fixed, in the negative direction of propagation, and that they are defined by the equations

$$V = -\kappa a S = m \sin \frac{2\pi}{\lambda} (r \cos \theta + \kappa a t + c_0).$$

At distances r from the centre of the sphere very small compared with λ , and very large compared with c the sphere's radius, the disturbance of the incident waves by the presence of the sphere is conceived to be of insensible magnitude. In this case the arc $\frac{2\pi r \cos \theta}{\lambda}$ is always very small within the extent of the disturbance, and it will suffice to expand the sine and cosine of it to terms inclusive of $\frac{r^2}{\lambda^2}$. Accordingly, putting q for $\frac{2\pi}{\lambda}$, the value of V may be put under the form

$$\begin{aligned} V = -\kappa a S = & m \sin q (\kappa a t + c_0) \\ & + m \cos q \kappa a t \left(q r \cos q c_0 \cos \theta - \frac{q^2 r^2}{2} \sin q c_0 \cos^2 \theta \right) \\ & - m \sin q \kappa a t \left(q r \sin q c_0 \cos \theta + \frac{q^2 r^2}{2} \cos q c_0 \cos^2 \theta \right). \end{aligned}$$

The terms involving qr are so small that their effects may be considered independently of each other, and also independently of the still smaller terms involving $q^2 r^2$. Taking, in the first place, the term containing $qr \sin q \kappa a t$, and substituting $-\kappa a \sigma_1$ for $m \sin q (\kappa a t + c_0)$, we have

$$S - \sigma_1 = \frac{m q r}{\kappa a} \sin q c_0 \sin q \kappa a t \cos \theta.$$

To this case the first of the two particular integrals treated of in the October Number is applicable, and we may at once adopt the expression for σ there obtained, which, when the arbitrary

constant c_0 is included, becomes

$$\sigma = \sigma_1 + 2m_1 \cdot \left\{ \frac{1}{r^2} \cos q(r + c_1) + \frac{q}{r} \sin q(r + c_1) \right\} \sin qkat \cos \theta.$$

The determination of the constant c_1 having been made on the same principle as before, to determine m_1 it is only necessary to satisfy the condition that $\sigma = S$ where r is so large that the sphere's influence is insensible, and consequently the terms involving its radius c may be omitted. After expanding $\sin qr$ and $\cos qr$, substituting the values of $\sin qc_1$ and $\cos qc_1$, and omitting terms of a negligible order, it will be found that

$$\sigma = \sigma_1 - \left(\frac{2m_1 q^3 r}{3} + \frac{m_1 q^3 c^3}{3r^2} \right) \sin qkat \cos \theta.$$

Hence, where r is very large compared to c ,

$$\sigma = \sigma_1 - \frac{2m_1 q^3 r}{3} \sin qkat \cos \theta.$$

Comparing this result with the above value of S , we have

$$m_1 = - \frac{3m \sin qc_0}{2kaq^2}.$$

Hence where $r = c$,

$$\begin{aligned} \sigma &= \sigma_1 - m_1 q^3 c \sin qkat \cos \theta \\ &= \sigma_1 + \frac{3qc \sin qc_0 \cos \theta}{2ka} \cdot m \sin qkat, \end{aligned}$$

which gives the condensation at the surface of the sphere so far as it is due to the term in the value of S which involves $qr \sin qkat$.

Let us now take account of the term involving

$$q^2 r^2 \cos^2 \theta \sin qkat.$$

The analytical reason for considering this term apart from the other, although it is of a higher order with respect to small quantities, is, that the particular integral in which it is involved satisfies the differential equation (η) in Part II. independently of the particular integral of the same equation which has just been employed. Our reasoning with respect to this new term will be precisely analogous to that applied to the other. In the first place, we have

$$S' = \sigma_1 + \frac{mq^2 r^2}{2} \cos qc_0 \sin qkat \cos^2 \theta,$$

and, by the same investigation as in the October Number,

$$\sigma' = \sigma_1 + m_1' \left\{ \left(\frac{1}{r^3} - \frac{q^2}{3r} \right) \cos q(r + c_1') + \frac{q}{r^2} \sin q(r + c_1') \right\} \sin qkat \cos^2 \theta.$$

Determining c_1' as before, expanding $\sin qr$ and $\cos qr$, and omitting insignificant terms, it will be found that

$$\sigma' = \sigma_1 - m_1' \left(\frac{2q^5 c^5}{135r^3} + \frac{q^5 r^2}{45} \right) \sin qkat \cos^2 \theta.$$

Hence where r is large we have

$$S' = \sigma_1 - \frac{m_1' q^5 r^2}{45} \sin qkat \cos^2 \theta.$$

Comparing this with the above value of S' , it follows that

$$m_1' = -\frac{45m \cos qc_0}{2q^3}.$$

Then putting c for r ,

$$\begin{aligned} \sigma' &= \sigma_1 - \frac{m_1' q^5 c^2}{27} \sin qkat \cos^2 \theta \\ &= \sigma_1 + \frac{5mq^2 c^2}{6} \cos qc_0 \sin qkat \cos^2 \theta, \end{aligned}$$

which gives the condensation at the surface of the sphere due to the term involving $q^2 r^2 \cos^2 \theta \sin qkat$.

It will be remarked that although the constants m_1 and m_1' have thus been determined, a new constant c_0 has been introduced, the character and value of which remain to be ascertained in order to complete the solution of the problem. This I hope to be able to do on a future occasion. At present I will only remark that if qc_0 be very small we may put in the small terms $\kappa a \sigma_1$ for $m \sin qkat$. In that case the expressions for σ and σ' will both have the form $\sigma_1(1 + mQ)$, and, by the same reasoning as that employed in Part II., it may be shown that to take into account the omitted quantity $\Delta\sigma_1$ it suffices to substitute $\sigma_1 + \Delta\sigma_1$ for σ_1 in the two formulæ. The tendency of the action of the waves to produce a permanent motion of translation of the sphere may then be deduced from the function $\Delta\sigma_1$ as before; and on the supposition that the waves emanate from a centre, the law of the inverse square is satisfactorily referable to the composition of this quantity, inasmuch as the number of *axes* of motion passing through a given area, to which the number of terms it involves has been shown to correspond, evidently varies inversely as the square of the distance. As my hydrodynamical researches have led me to conclude that the velocity (V) in central motion varies inversely as the square of the distance, the quantity $\frac{V^2}{\kappa a}$, to which $\Delta\sigma_1$ in the former investigation was supposed to be equal, would for such motion vary inversely as the *fourth* power of the distance. This particular being excepted, I have seen no reason either to correct, or to add to, the remarks made in Part II. relative to the bearing of these Researches on a mathematical theory of attractive and repulsive forces.

I have further to state that, having gone through investigations analogous to the two preceding, relative to the two terms

in the expression for V which contain $\cos qkat$, I obtained corresponding values of $\sigma - \sigma_1$ and $\sigma' - \sigma_1$. In case qc_0 be small, these values involve as a factor $\frac{d\sigma_1}{dt} + \frac{d \cdot \Delta\sigma_1}{dt}$, and consequently, since $\frac{d \cdot \Delta\sigma_1}{dt}$ is wholly periodic, only indicate a tendency of the action of the waves to produce *vibrations* of the sphere. For this reason the results can in no case have any bearing on the main object of my present researches, and I have therefore not thought it worth while to exhibit them. It may, however, be mentioned, as being confirmatory of the correctness of the mathematical processes I have employed, that the investigation relative to the term involving $qr \cos qkat$, led to results respecting the mutual action of the sphere and fluid in accordance with those obtained by the usual treatment of the problem of the resistance of the air to a ball-pendulum. But, for reasons which I need not now repeat, I am of opinion that both these solutions relate to circumstances of the motion different from those contemplated in the problem which has been under discussion in this communication.

Cambridge, December 21, 1865.

VI. *On Earthquakes and extraordinary Agitations of the Sea.*

By RICHARD EDMONDS, *Esq.**

AS no account of the extraordinary disturbance of the sea at Plymouth and Penzance on the 14th of October, 1862, has yet appeared except in the newspapers, I desire now to record it.

Accompanied by a thunder-storm, as usual with such disturbances, it was first observed about sunrise, when the sea in each place rose four or five feet above its proper level, and the alternating current (rushing in and rushing out) was so strong and of such long continuance in Plymouth that the large iron buoy outside the gates of the Great Western Docks was frequently, to the astonishment of the beholders, completely submerged; and the gates could not be shut all that day, nor during the following night. The same morning at 7.30, in the Truro river (a branch of Falmouth harbour), "when the tide had been ebbing there about three hours, and had almost left the wharf occupied by Mr. John James, a sudden wave or 'bore' came up the river, the water rising from three to four feet, and coming up

* Read before the Royal Geological Society of Cornwall, on the 24th of October, 1865, and communicated by the Author.

at the rate of five to six miles an hour, carrying a lighter, which was previously aground at Mr. James's wharf, entirely up to the Truro quay, a circumstance of very extraordinary character"*.

On the 17th (three days afterwards) a similar phenomenon was observed at Liverpool. All these happened during that week of terrific storms ending on Sunday night, the 19th, when the barometer was lower than it had been for three years before. This week is still more memorable from being in the midst of the nineteen days of earthquakes which desolated the island of Fayal in the Azorean group.

It is a fact worthy of investigation, that when a phenomenon of this kind occurs at the ports of Mountsbay, a similar occurrence always takes place, as far as I have been able to ascertain, contemporaneously at Plymouth; and probably also in the intermediate harbours of Falmouth and Fowey,—and this, too, whether such agitation be noticed elsewhere or not. The days in the present century on which these disturbances were observed, both in Mountsbay and in Plymouth, are, 31st May and 8th June, 1811; 5th July and 30th October, 1843; 23rd May, 1847; 25–26th June and 4th October, 1859; and 14th October, 1862,—the agitations, although not accompanied with known earthquakes, being *precisely* of the same character as those in Mountsbay and Plymouth on the days of the great earthquakes of 1755 and 1761; except that at Newlyn, in the north-west corner of Mountsbay, in 1755 the sea “came on like a surge or high-crested wave”†; and that in Lamorna Cove, three miles south of Newlyn, in the same bay, the sea, as I have been informed by two descendants of an eye-witness, rushed to the shore in enormous waves, sweeping along blocks of granite weighing several tons each, and leaving some of them eight or ten feet above the level of spring tides. On the 31st of May, 1811, above mentioned, the agitation at Plymouth (says Mr. Luke Howard) “commenced about 3 A.M. and did not terminate till 10. The sea fell instantaneously about four feet, and immediately rose about eight feet. Universal consternation pervaded the whole port. The vessels in Catwater were thrown about in the greatest confusion; many dragged their anchors, some drifted, and several lost their bowsprits and yards. About 6.45 the sea rose to the height of eleven feet and again receded”‡. In Mountsbay, on the day of the great earthquake of Lisbon, the perpendicular rise of the sea at Penzance pier was eight feet, but at Newlyn pier not less than ten feet. These tide-like alterna-

* West Briton of 24th October, 1862.

† Phil. Trans. vol. xlix. p. 373.

‡ See Edinb. Phil. Trans. vol. xv. p. 618.

ting currents occupy generally, in each efflux, as well as in each influx, from five to ten minutes.

Borlase, in his 'Natural History of Cornwall,' as well as in the Transactions of the Royal Society, has described similar disturbances in Mountsbay during the last century. And I have in the 'Transactions' of this Society given descriptions of several in the present century, which are now recorded also in my work 'On the Land's-end District,' published in 1862.

To explain these phenomena when accompanied by known earthquakes, Mr. Mallet has written an article in the 'Quarterly Journal of Science' for January 1864 (No. I. p. 68), into which journal was merged that year the 'Edinburgh New Philosophical Journal,' which contained my numerous papers on the subject, read before the Society during the years 1843-60. The following is his hypothesis:—

"Should the origin of the earthquake be *under the sea*, then at the point passed through by the seismic vertical, and around it, the sea-bottom is, as on land, suddenly upheaved and again dropped down; or it may be, as by a submarine volcano, actually broken up altogether, and steam, lava, and floods of lapilli, &c. may then be belched out under water. In *either case* there is forced up a volume of water upon the sea's surface just above, or several of these in succession; and as each mass falls again it assumes the horizontal form of a circular liquid wave of translation; and these are propagated outwards over the surface of the sea, like the circles or ring-shaped waves on a pond when a pebble is dropped into it. The altitude and breadth of these waves depend mainly upon the magnitude of the disturbance at the bottom, and on the depth of water above it: the rate of their propagation outwards has nothing to do directly with elasticity; it is dependent simply upon the square root of the depth of the water traversed by the wave on its surface. * * * * When the long flat swell of such waves as they are originated in the deep sea approaches the shores and reaches shoal waters, their fronts become sharper and steeper, and they finally roll in upon the shore as the *great sea-waves* of South American and other earthquakes so much dreaded. * * * The *great sea-wave* of translation rolls in, often hours after the shock has done its work of destruction; or portions of it may roll in upon shores that have felt no shock at all. Thus in the great earthquake at Japan which a few years ago wrecked a Russian frigate in one of the harbours there, the great sea-wave produced in the deep sea near those islands, hours afterwards reached the opposite shores of the Pacific, at St. Diego and Francisco."

This hypothesis, although advanced by the most eminent English writer on earthquake phenomena, appears to me highly

improbable. Assuming (as the passage quoted allows) that there is no "submarine volcano," nor any dislocation or fracture of the submarine ground, a violent shock proceeding upwards vertically from a horizontal portion of the basin of the sea would pass through the sea as through a solid, without displacing any water until it reached the surface, which would be then dashed up. If a ship were floating on the spot, that part of the surface immediately under her would transmit the shock to the ship, and loose articles on her deck would be jerked up to heights proportioned to the violence of the shock. Thus on board a ship 40 leagues west of St. Vincent, the men were thrown "a foot and a half perpendicularly up from the deck" *.

If the submarine ground, instead of being horizontal, were an inclined plane forming the steep side of a mountain, a shock proceeding vertically from the interior of the earth would, on leaving this inclined plane, pass through the sea in a direction perpendicular to the plane; and when it reached the surface of the sea, that surface would be dashed up in the same direction. If the shock were moving westward, a ship receiving it while sailing eastward would be stopped as suddenly as if she had struck on a rock. Instances of this are also on record.

Now let us imagine some shore descending, at an angle of 45° , to the depth of a furlong or two under the sea. If this received a vertical shock, the sea resting thereon would transmit the shock to the surface of the water, which surface would be then dashed seaward. If the shock were not repeated, the water driven seaward would instantly flow back towards the shore to regain its level. But if countless shocks or vibrations followed in rapid succession (as is commonly the case in earthquakes; for a "shock" is generally a rapid succession of vibrations like those produced by letting out a ship's cable †), another dashing seaward of the surface would take place, followed by countless others, until the water thus driven on would be raised into a considerable heap; so that when the vibrations or shock ceased, it would flow back towards the shore and rise perhaps above its previous level. This

* Lyell's 'Geology,' vol. ii. p. 241, 3rd edit.

† In reading descriptions of earthquakes felt *under ground*, *above ground*, and *at sea*, I have been struck with the three classes of comparisons by which the noises accompanying them have been represented. Persons *under ground* have compared the noise to that of "the breaking of a 'studdle' (wooden platform), and the 'deads' (rubbish) being set a running," and to "the falling of a 'kibble' (bucket) down the shaft." Those *above ground* likened it to "the rolling of stones," and "the rushing of waggons down a road." Sailors *at sea* compare it to "the letting out of the cable." All these comparisons, varying according to the situation of the observer, show that the "shock" consists of a rapid succession of vibrations.

accounts for the first efflux and the first influx of these extraordinary oscillations, the succeeding effluxes and influxes being merely as the continued oscillation of a pendulum when once set in motion. Thus we see why these agitations generally *commence* with an *efflux* on our coasts and in our lakes; and why the waters rise into heaps in the centres of lakes, and then flow back to their shores.

Sometimes during these oscillations of the sea subsequent submarine shocks interfere with the effects of preceding ones, and retreating currents meeting advancing ones may occasion those "mountainous breakers" (as Darwin calls them), or those "great sea-waves" (as Mallet calls them), which occurred at Newlyn and Lamorna in 1755, as already stated. Nor is it unusual for a shock on dry land to be succeeded by another after a short interval; indeed each of the shocks in Cornwall on the 21st of October, 1859, and the 13th of January, 1860, was followed by a second shock about an hour afterwards—the former at Truro, the latter near Liskeard.

I am unable to understand how any "long flat swell," moving onwards according to the laws of ordinary waves, and arising, as Mr. Mallet supposes, in the deep sea, or from a "submarine volcano" hundreds, nay, thousands of miles off, but which has never been observed, could ever have produced such agitations on our coasts or in our harbours. How, for example, could it have produced such and so frequent disturbances at the piers of Penzance and St. Michael's Mount, only two miles apart, and yet none has been observed on the broad and almost horizontal shore midway between them? Or how could this "long flat wave" or those successive "ring-shaped waves" propagated from a centre, the "seismic vertical" in the deep ocean near Japan, and moving towards all the points of the compass, produce such effects in a Japanese harbour and likewise in some American harbours nearly 5000 miles off, without occasioning similar effects also in most of the harbours within 2000 miles of Japan? Besides, if this "long flat wave" had any existence, it would invariably, the very moment it reached the shore, begin to flow up the beach without any previous efflux; whereas these agitations begin generally with an efflux lasting from five to ten minutes, as did that at Plymouth, of 31st May, 1811, already described, that at Penzance, of 5th July, 1843*, and that at St. Mary's pier, Scilly, of 4th October, 1859†.

Judging from what has been actually observed, there seems

* See my letter describing this in the 'Literary Gazette' of 15th July, 1843, p. 464.

† See the Tide-gauger's report in my work 'On the Land's-end District,' p. 88.

reason to conclude that on these occasions, as well as on the day of the great earthquake of 1755, every disturbance of the sea, wherever it happened, was confined to a few furlongs from that part of the bed of the sea over or near which the agitated waters had previously rested; so that all such disturbances in 1755, although observed the same day on most of the European coasts, were perfectly independent of one another, and proceeded not from any distant disturbance in mid-ocean, but from local submarine shocks beneath or close to the disturbed waters. In Falmouth harbour and Plymouth and Mountsbay, for instance, where such extraordinary currents have so often simultaneously occurred, no kind of disturbance has been observed in the offing*. Indeed I have ascertained by personal inquiries from eye-witnesses and other sources, whilst residing in Penzance, that such agitations at the piers of St. Michael's Mount, Penzance, and Newlyn (the first being two miles east, and the last one mile west of Penzance pier) have always been independent of each other, and none of them ever extended beyond a few furlongs from the pier where it was observed.

Assuming that the extraordinary disturbances of the sea in Plymouth and Mountsbay on the days of the two great earthquakes of 1755 and 1761 were occasioned by submarine shocks, and that those in the same places in 1811, 1843, 1847, 1859, and 1862 were in all respects like them (neither of which facts will be questioned), the only reasonable conclusion is, that all these *like effects* must have resulted from *like causes*—that is, from submarine shocks, whether any shocks on dry land were then perceived at those places or not. This may appear more clearly by considering some of the phenomena of the earthquake of 1755.

If (as is universally allowed) the centre of this great earthquake was deep in the interior of the earth, most of the shocks therefrom which reached the surface must have proceeded either vertically, or upwards at various angles, and the times of reaching the surface at different places must have been generally according to their distances, or the angles of propagation, and the conducting-power of the intervening ground. The shocks which moved through very bad conductors might have been exhausted long before they could attain the surface. Some reached as far upwards as the bottom of the sea and the basins of lakes and ponds, occasioning extraordinary disturbances of the waters; and yet most of them fell short of the surface of the adjoining dry lands. Whilst five smart shocks were felt in Derbyshire Peak between 11 and 11.20 A.M., sixty fathoms under ground, only one reached the surface there. Mr. Mallet mentions this and two places

* So also at Swansea, in 1755 (Phil. Trans. vol. xlix. p. 379).

near Reading as the only places where shocks were felt in this island; but there is abundant traditional evidence of shocks having been felt in Cornwall; whilst in the Scilly Isles "several persons ran out of their houses for fear they would fall upon them"*.

On the other hand, if the centre of an earthquake be inland and close beneath the surface, the shock would not travel vertically or upwards at all, but horizontally, as the shock in this country of the 6th of October, 1863, appears to have done; for when at Pontypool, in the midst of Welsh mines, soon after its occurrence, I was informed by a gentleman who had ascertained the fact from the miners themselves, that whilst the shock was felt at the mines *above* ground, it was not felt by any *under* ground. Nor does it appear to have been felt *under* ground in any of the mines of Cornwall†.

The two preceding paragraphs may help to account for a striking fact mentioned by Mr. Mallet, viz. that the extraordinary disturbances of the sea now under consideration have "never been observed to take place in any earthquake whose centre of impulse was inland, however violent"‡. This fact (which is in perfect agreement with my hypothesis) may perhaps be owing to the centre of impulse, when inland, being generally close under the surface, so that the shocks must travel horizontally, or nearly so; and it is not to horizontal, but to vertical shocks that I have always ascribed these disturbances of the sea.

Another fact not less remarkable is, that, while earthquakes in general take place equally in all states of the atmosphere, those which are known only by the extraordinary agitations of the sea or lakes which they produce occur almost exclusively during storms, or at or near considerable minima of the barometer. Is it because these submarine shocks, as already stated, are almost always vertical, or nearly so, while those on dry land are generally horizontal? In *vertical* shocks there may be electrical discharges between the earth and the atmosphere which might occasion the attendant minima of the barometer, whereas in *horizontal* shocks the discharges (if any) may be only between differently charged portions of the earth without much affecting the atmosphere. The first earthquake felt by Humboldt in Cumana was during a severe thunder-storm. "At the moment of the strongest electrical explosion were two considerable shocks of an earthquake;" but the barometer, which had been previously falling, continued to fall for five hours afterwards, when a third and last shock

* Troutbeck's 'Scilly,' 1794, p. 40.

† Journal of the Royal Institution of Cornwall, No. 1. p. 61.

‡ British Association Reports, vol. xx. p. 46.

occurred, at which "moment the mercury was precisely at its minimum height"*.

Having in 1843, at the request of Sir Charles Lemon, the then President, written an account of the extraordinary oscillations of the sea of the 5th of July in that year, I have considered it due to this Society, as well as to myself, to state thus fully my reasons for still regarding the hypothesis I then advanced as the only one capable of reconciling all the known facts connected with these disturbances.

VII. *Astronomical Prolusions: commencing with an instantaneous proof of Lambert's and Euler's Theorems, and modulating through a construction of the orbit of a heavenly body from two heliocentric distances, the subtended chord, and the periodic time, and the focal theory of Cartesian Ovals, into a discussion of motion in a circle and its relation to planetary motion.* By J. J. SYLVESTER, F.R.S.†

THE original demonstration by Lambert of the celebrated theorem which bears his name was a geometrical one. See Monthly Notices of the Astronomical Society, vol. xxii. p. 238, where this demonstration is reproduced, or rather recapitulated by Mr. Cayley. See also Lambert's own *Insigniores Orbitæ cometarum proprietates*, Augusta Vindelicorum [Augsburg], 1761. It occupies seven or eight pages of this celebrated tract, and, elegant as may be considered the chain of geometrical enunciations from which it is deduced, is, as a specimen of geometrical style, little worthy of the inconsiderate commendations which have been heaped upon it, containing, as it does, a hybrid mixture of algebraical, geometrical, and trigonometrical ratiocination. The late Professor MacCullagh, as I am informed by my ingenious coadjutor Mr. Crofton, one of his hearers at Trinity College, Dublin, greatly improved upon Lambert's method, and succeeded in reducing it to a purely geometrical form. Lagrange has given no less than four distinct demonstrations of the same,—

* Personal Narrative, vol. iii. pp. 316-318.

† Communicated by the Author. A portion of this paper has appeared in the Monthly Notices of the Astronomical Society of London for December last, viz. so much of it as relates to Lambert's theorem *proper*. The portion concerning circular motion formed the subject of a communication to the London Mathematical Society at the Meeting of December 18, 1865. The part which presented itself last to the author's mind, and is consequently the least developed, is that which relates to the determination of the forces in any orbit to any two (or more) centres of force. The general expression for such forces will be found stated further on in a footnote, where the *equation of radial work* is defined and employed to obtain the solution in a form of unexpected simplicity.

one a sort of verification by aid of trigonometrical formulæ in which the eccentric anomalies are introduced; a second of a similar nature, but dealing only with the true anomalies; a third founded on a property of integrals*; and a fourth, perhaps the most remarkable of any, derived from the general expressions for the time in an orbit described about two centres of force varying according to the law of nature, but one of them supposed to be situated in the orbit itself, and to become zero. Notwithstanding this plethora of demonstrations I venture to add a seventh, the simplest, briefest, and most natural of all, in which I employ a direct method to prove, from the ordinary formulæ for the time of a planet passing from one point to another, that, when the period is given, the time is a function only of the sum of the distances of these points from the centre of force, and of their distance from one another.

Let ρ, ρ' be the distances of the two positions from the sun, c their distance from one another, v, v' the true, u, u' the eccentric, m, m' the mean anomalies thereunto corresponding, e the eccentricity, $\omega = m - m'$, $s = \rho + \rho'$, $\Delta = \frac{1}{2}(s^2 - c^2)$; then

$$\rho = 1 - e \cos u, \quad \rho' = 1 - e \cos u', \quad m = u - e \sin u, \quad m' = u' - e \sin u',$$

$$\rho \cos v = \cos u - e, \quad \rho \sin v = \sqrt{1 - e^2} \sin u,$$

$$\rho' \cos v' = \cos u' - e, \quad \rho' \sin v' = \sqrt{1 - e^2} \sin u',$$

$$c^2 = \rho^2 + \rho'^2 - 2\rho\rho' \cos(v' - v).$$

Writing for brevity p, p', q, q' for $\cos u, \cos u', \sin u, \sin u'$, we have

$$s = 2 - ep, -ep', \quad \omega = u - u' - eq + eq',$$

$$\Delta = \rho\rho' + \rho\rho' \cos(v' - v) = 1 + p, p' + q, q' - 2e(q + q') + e^2(1 - q, q' - pp').$$

Let $J = \frac{d(\Delta, s, \omega)}{d(e, u, u')}$; then J is the determinant,

$$\begin{vmatrix} \begin{Bmatrix} -2(p+p') \\ +2e(1+pp'-qq') \end{Bmatrix} & \begin{Bmatrix} p'q-pq'+2pq \\ -e^2(pq'+p'q) \end{Bmatrix} & \begin{Bmatrix} pq'-p'q+2p'q' \\ -e^2(pq'+p'q) \end{Bmatrix} \\ -p-p' & eq & eq' \\ -q+q' & 1-ep & -1+ep' \end{vmatrix}$$

* The property in question, discovered by Lagrange, is that the integral

$$\int \frac{rdr}{\sqrt{H+Mr+Nr^2}}$$

may be transformed into

$$\int \frac{(x^2+h)dx}{\sqrt{a+bx+cx^2+dx^3+ex^3}} - \int \frac{(y^2+h)dy}{\sqrt{a+by+cy^2+dy^3+ey^4}};$$

in applying it to Lambert's theorem a, b, c are made to vanish. This transformation and its consequences appear to us to deserve further study; as far as I know it has not been touched upon by the writers on elliptic functions.

Denoting this determinant by $\begin{vmatrix} A, B, C \\ D, E, F \\ G, H, K \end{vmatrix}$,

we find

$$(A, B, C) - 2H(D, E, F) + 2E(G, H, K) = (0, B, -B),$$

$$(A, B, C) - 2K(D, E, F) + 2F(G, H, K) = (0, -C, C),$$

$$\text{so that } J = \begin{vmatrix} A, B, C \\ 0, B, -B \\ 0, -C, C \end{vmatrix} = 0.$$

Hence it appears that $d\omega$ is a linear function of ds and $d\Delta$; that is, ω is a function of s and Δ , or, what is the same thing, of s and c , and independent of e . If then, when $e=1$, the corresponding values of $\rho, \rho', v, v', u, u'$ are $r, r', \theta, \theta', \phi, \phi'$, we have $\cos \theta = -1$, $\cos \theta' = -1$, $\sin \theta = 0$, $\sin \theta' = 0$, $r - r' = c$, $r + r' = s$, whence, writing

$$1 - \cos \phi = \frac{s+c}{2}, \quad 1 - \cos \phi' = \frac{s-c}{2},$$

we have finally $\omega = \phi - \phi' - \sin \phi + \sin \phi'$, as was to be proved.

Essentially this demonstration is of the same nature as the first of Lagrange's four methods of proof above referred to, but with the difference that it leads up to and accounts beforehand for the success of the transformations therein employed.

Alluding to Lambert's cumbrous demonstration, Lagrange says of it, "His theorem merits the especial notice of mathematicians, both on its own account, and because it appears difficult to arrive at it by algebraical processes (*calcul*); so that it may be ranked among the small number of those in which geometrical seems to have the advantage over algebraical analysis." In the nature of things such advantage can never be otherwise than temporary. Geometry may sometimes appear to take the lead of analysis, but in fact precedes it only as a servant goes before his master to clear the path and light him on his way. The interval between the two is as wide as between empiricism and science, as between the understanding and the reason, or as between the finite and the infinite.

The result so simply obtained above is of course not restricted to the case of the ellipse, but applies to motion generally about a centre of force according to the law of nature.

Calling t the time, the syzygy shown to exist between $\delta t, \delta s, \delta c$, being independent of any supposition as to the value of e , or as to the reality of the functions employed, will of necessity

continue to obtain where, e being greater than 1, the motion becomes hyperbolic. If μ be the absolute force, and 1, as before, the semi-major axis, writing

$$\epsilon = \sqrt{\frac{e-1}{e+1}}, \quad \epsilon \tan \frac{v}{2} = x, \quad \epsilon \tan \frac{v'}{2} = x',$$

the rest of the notation being preserved, we obtain, by direct integration and substitution,

$$\frac{s}{2} = \frac{1}{1-\epsilon^2} \left(\frac{\epsilon^2 + x^2}{1-x^2} + \frac{\epsilon^2 + x'^2}{1-x'^2} \right),$$

$$\frac{\Delta}{8} = \frac{1}{(1-\epsilon^2)^2} \frac{(\epsilon^2 + x^2)(\epsilon^2 + x'^2)}{(1-x^2)(1-x'^2)},$$

$$t = \mu^{\frac{1}{2}} \left\{ \log \frac{1-x'}{1+x'} - \log \frac{1-x}{1+x} + 2(x-x')(1-xx') \right\}.$$

And we must needs find by actual computation

$$\text{the Jacobian } \frac{d(s, \Delta, t)}{d(\epsilon, x, x')} = 0.$$

Making $\epsilon=0$, and giving x, x' their corresponding values in terms of s and Δ , there results

$$\frac{x^2}{1-x^2} = \frac{s+c}{2}; \quad \frac{x'^2}{1-x'^2} = \frac{s-c}{2};$$

and accordingly

$$t = \mu^{\frac{1}{2}} \left\{ \log (\sqrt{s+c+2} - \sqrt{s-c}) - \log (\sqrt{s-c-2} - \sqrt{s-c}) \right. \\ \left. - \sqrt{(s+c+1)^2-1} + \sqrt{(s-c+1)^2-1} \right\}.$$

It is worthy of notice that the effect of making $\epsilon=0$ or $e=-1$ in this case, like that of making $e=1$ in the case of elliptic motion, is to reduce the motion to that of a body in a straight line, but with the difference that for the elliptic the reduced motion is that of a body moving between the point of instantaneous rest and the centre of force or point of infinite velocity, whereas for the hyperbola it is that of a body moving on the same side of these two points.

The theorem for the case of the parabola was given by Euler (1744), but reproduced independently by Lambert in the *Insigniores Proprietates*, Sectiones 1, 2, in 1761.

I think the idea of the general theorem may not unlikely have originated in an observation of the accordance in form of the result for parabolic motion with that for motion in a straight line, an accordance easily verified to extend to motion in a circle.

Such coincidence, to a mind open to the impressions of analogy, could hardly fail to suggest the necessity of the existence of a deeper-seated law, of which these extreme cases must represent the outcroppings. Euler's theorem is of course included as a particular case in Lambert's, and may be derived from it by making a infinite in the expression for t as a function of s, c, a ; but it may also be obtained independently as follows. Calling $4m$ the latus rectum, retaining the rest of the notation, and writing $\tan \frac{v}{2} = q, \tan \frac{v'}{2} = q'$, we easily find

$$\begin{aligned}\frac{1}{2}\sqrt{\Delta} &= m(1 + qq'), \\ s &= m(2 + q^2 + q'^2), \\ \frac{t}{\sqrt{2}} &= m^{\frac{3}{2}}\left((q - q') + \frac{1}{3}(q^3 - q'^3)\right).\end{aligned}$$

Hence the Jacobian

$$\frac{d(\frac{1}{2}\sqrt{\Delta}, s, \sqrt{2} \cdot t)}{d(m, q, q')}$$

becomes a multiple of the determinant,

$$\begin{vmatrix} 1 + qq'; & q'; & q \\ 2 + q^2 + q'^2; & 2q; & 2q' \\ 3(q - q') + q^3 - q'^3; & 2 + 2q^2; & -2 - 2q^2 \end{vmatrix}$$

Calling this

$$\begin{vmatrix} A & B & C \\ D & E & F \\ G & H & K \end{vmatrix}$$

it will be found that

$$(A; B; C) - \frac{H}{2A - F}(D; E; F) + \frac{E}{2A - F}(G; H; K) = 0; -B;$$

$$(A; B; C) + \frac{K}{2A + E}(D; E; F) - \frac{F}{2A + E}(G; H; K) = 0; -C; C;$$

and consequently the Jacobian in question, as before, takes the form

$$\begin{vmatrix} A; & B; & C \\ 0; & B; & -B \\ 0; & -C; & C \end{vmatrix}$$

which is identically zero; so that t is a function only of s, c when a is given, and one solution is left free between m, q, q' .

Making $q = \infty$, we have

$$m(q-q') = \sqrt{s-\sqrt{\Delta}} = \frac{\sqrt{s+c} - \sqrt{s-c}}{2},$$

$$m(q+q') = \sqrt{s+\sqrt{\Delta}} = \frac{\sqrt{s+c} + \sqrt{s-c}}{2},$$

$$mq = \frac{\sqrt{s+c}}{2}, \quad mq' = \frac{\sqrt{s-c}}{2};$$

and thus

$$t = \frac{1}{6}((s+c)^{\frac{3}{2}} - (s-c)^{\frac{3}{2}}).$$

There is a sort of pendant, to Lambert's theorem which is worthy of notice. If we call $\rho - \rho' = v$ and $c^2 - \sigma^2 = D$, writing

$$ae(\sin u' - \sin u) = \Omega,$$

we have also

$$(1-e^2)a^2(1-\cos(u'-u)) = D,$$

$$ea(\cos u - \cos u') = \sigma,$$

from which we easily obtain

$$\Omega = \sqrt{\frac{e^2 c^2 - \sigma^2}{1-e^2}};$$

so that Ω is a function only of c, e, σ , as by Lambert's theorem it is a function only of c, a, s . Moreover, since

$$\sin\left(\frac{u'-u}{2}\right) = \sqrt{\frac{D}{2(1-e^2)}},$$

it is apparent that the change in the mean anomaly is a complete function of the two variables $\frac{\sqrt{D}}{b}, \frac{c}{a}$, as by Lambert's theorem it is of the two $\frac{\sqrt{\Delta}}{a}, \frac{c}{a}$. Comparing the value of Ω given immediately above with that which is contained in Lambert's theorem, the solution of a linear equation leads immediately, after certain simple reductions, to the equation

$$1-e^2 = \frac{2(c^2-\sigma^2)}{ss'+c^2 \pm \sqrt{(s^2-c^2)(s'^2-c^2)}},$$

where $s'+s=4a$. And as there is nothing to determine the signs of ρ or ρ' , the above, by interchanging severally and independently ρ, ρ' with $-\rho, -\rho'$, represents eight values of e :—four corresponding to the change of ρ into $-\rho$, and ρ' into $-\rho'$, contained in the expression immediately above written, combined with the equation $s'+s=\pm 4a$; and four in the conjugate ex-

pression

$$1 - e^2 = \frac{2(c^2 - s^2)}{\sigma\sigma' + c^2 \pm \sqrt{(c^2 - \sigma^2)(c^2 - \sigma'^2)}},$$

where $\sigma' + \sigma = \pm 4a$.

Since we have also (calling i the angle between c and a) $\cos i = \frac{\sigma}{ec}$ in the first case, and $\cos i = \frac{s}{ec}$ in the second case, the problem of determining the conic, of which one focus, the major axis, and two points are given, is thus completely solved. This of course comprehends the analytical solution of the problem of determining the magnitude and position of the orbit of a planet from the periodic time, two heliocentric distances, and the included angle, of which no mention is to be found in any of the ordinary books of astronomy, or even in the *Theoria Motus*, where one would naturally expect to find it.

There are thus eight values of e^2 , and the solution is eight-fold. The sign of $\cos i$ being left ambiguous does not raise the number to 16; for this ambiguity depends upon the fact of the direction of c being incapable of analytical representation; only one of these values of $\cos i$ will appertain to any stated case. If F be the given focus, P, Q the two given points, and G the second focus, by rotating the figure about the line FG P, Q come into the positions P', Q' ; c, s, σ remain unaltered; but the angles between $Q'P', FG$, and between QP, FG become supplementary. If we chose to effect a direct solution of the problem of determining the orbit without the aid of the eccentric anomalies, we should have to eliminate θ, θ' between the equations

$$\pm \rho = \frac{a(1 - e^2)}{1 - e \cos \theta}; \quad \pm \rho' = \frac{a(1 - e^2)}{1 - e \cos \theta'}; \quad c^2 = \rho^2 + \rho'^2 - 2\rho\rho' \cos(\theta - \theta').$$

This elimination will be found to lead to a quadratic equation in e^2 , the coefficients of e^6 and e^8 vanishing; and we thus obtain an eightfold solution as before, but in a more involved form. Or, again, we might write

$$(a(1 - e^2) + ex)^2 = \rho^2,$$

$$(a(1 - e^2) + ex')^2 = \rho'^2,$$

$$xx' + yy' = c^2,$$

$$x^2 + y^2 = \rho^2, \quad x'^2 + y'^2 = \rho'^2,$$

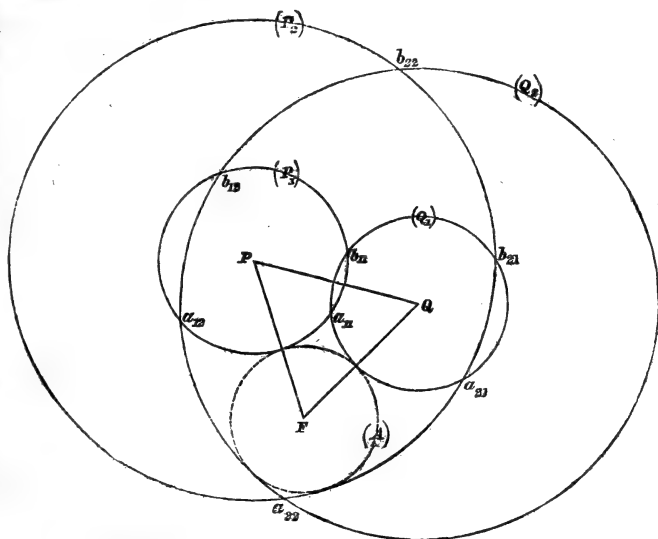
and between these five equations eliminate x, x', y, y' . By the general theory of elimination, e^2 should rise to the sixteenth degree in the resultant; but in fact it will rise only to the eighth. The following obvious geometrical construction will

perfectly account *à priori* for the existence of the excluded infinite values of e^2 .

Since $FP \pm GP = \pm 2a$ and $FQ \pm GQ = \pm 2a$,

G will be any point in the intersection of either of two circles C_1, C_2 with either of the two K_1, K_2 , where the squared radii of C_1, C_2 are $(2a + FP)^2$, and of K_1, K_2 $(2a + FQ)^2, (2a - FQ)^2$ respectively. Consequently there are eight real or imaginary positions of G at a finite, and eight at an infinite distance.

It is obvious that, if we restrict the orbit to being elliptical, there can never be more than two admissible solutions; but treating the question more generally, any even number of solutions whatever may exist from 0 to 8, both inclusive. I am indebted to my able friend Dr. Hirst for the following figure, illustrating the interesting case where all eight solutions are real and hyperbolæ.



In this figure

$$\rho (= FP) > 2a, \quad \rho' (= FQ) > 2a,$$

and likewise

$$\rho + \rho' - 4a > c,$$

$$4a - \rho + \rho' < c,$$

$$4a + \rho - \rho' < c.$$

Supposing FP, FQ to be each greater than $2a$, there will be no difficulty in verifying the following statement.

One pair of hyperbolæ, in which P, Q lie in the branch containing F , will be always real; a second pair, in which they lie

in the opposite branch, will be real or imaginary according as s is greater or less than $c+4a$, *i. e.* according as $2a$ is less or greater than $\frac{s-c}{2}$. A third pair, in which the two given points are distributed between the two branches, will be real or imaginary respectively according as $2a$ is less or greater than $\frac{c+\sigma}{2}$; and a fourth pair, where the same distribution occurs, will be real or imaginary according as $2a$ is greater or less than $\frac{c-\sigma}{2}$.

It is of course only with the first kind of hyperbolæ, that in which the given points lie in the branch concave to the given focus, with which the problem, regarded as an astronomical one, is concerned. But in all cases the formulæ given for the determination of e and i admit of immediate adaptation to logarithmic computation. Thus, *ex. gr.*, if we take the one which meets the case of distribution between the two branches of an hyperbola, viz.

$$e^2 - 1 = \frac{2(s^2 - c^2)}{(\sigma\sigma' + c^2) \pm \sqrt{(c^2 - \sigma^2)(c^2 - \sigma'^2)}}$$

writing

$$e = \sec \phi, \quad s = c \sec \lambda, \quad \sigma = c \cos \mu, \quad \sigma' = c \cos \mu',$$

we obtain

$$\tan \phi = \tan \lambda \sec \frac{\mu \pm \mu'}{2}$$

$$\pm \cos i = \cos \mu \cos \phi.$$

Viewed as a question of analytical geometry, the investigation as to the reality of the curve would have to be treated in a more general manner; *i. e.* without assuming, as I have done, the necessity of the inequalities $s > \rho_1 + \rho_2$, $\sigma > \rho_1 - \rho_2$, where ρ_1, ρ_2 represent the two given focal distances; for it is a very important, although hitherto strangely neglected geometrical principle, that every real conic is at one and the same time an ellipse and hyperbola; viz. either an actual ellipse accompanied by an ideal hyperbola, or an actual hyperbola accompanied by an ideal ellipse. This may immediately be made manifest by considering how the ordinary rectangular-coordinate equation to a conic, with its origin transferred to a focus, is connected with the property of the conic in respect to its two foci. Calling ρ, ρ' the two focal distances of any point, the equation to rectangular coordinates is obtainable by equating to zero the norm of the quantity $2a \pm \rho \pm \rho'$, where ρ represents $\sqrt{x^2 + y^2}$, and ρ' represents $\sqrt{(2ap + x)^2 + y^2}$, which norm will only be of the *second degree* in x, y , although a product of four factors each of the first degree in x, y , owing to

the vanishing of the coefficients of the terms that ought to rise to the fourth degree in the variables. Calling, then, this norm N , we see that the quadratic equation $N=0$ is satisfied alike by the equations $\rho + \rho' = 2a$ and $\rho - \rho' = 2a$, the difference being that one of these will belong to the apex of an actual, and the other to that of an ideal triangle, according to the sign of $e-1$.

It may not be quite out of place here to show how immediately the knowledge of the existence of a third focus to the Cartesian ovals, that remarkable discovery of our illustrious Royal Society *Laureate* of the year, flows from a similar process to the one above. For taking the norm of the expression

$$a \sqrt{x^2 + y^2} + \sqrt{b^2 x^2 + b^2 y^2 + 2bcx + c^2 + d^2},$$

the equation to any such curve becomes

$$\begin{aligned} & ((b^2 - a^2)(x^2 + y^2) + 2bcx + c^2)^2 \\ & - 2d^2((b^2 + a^2)(x^2 + y^2) + 2bcx + c^2) \\ & + d^4 = 0, \end{aligned}$$

$$\begin{aligned} \text{i. e.} \quad & (b^2 - a^2)^2(x^2 + y^2)^2 + 4bc(b^2 - a^2)x(x^2 + y^2) \\ & + (2(c^2 - d^2)b^2 - 2(c^2 + d^2)a^2)(x^2 + y^2) \\ & + 4b^2c^2x^2 + 4(c^2 - d^2)bcx + (c^2 - d^2)^2 = 0; \end{aligned}$$

in which equation a^2, b^2, c^2, d^2 may obviously be replaced by $\alpha^2, \beta^2, \gamma^2, \delta^2$, provided

$$\left. \begin{aligned} \beta^2 - \alpha^2 &= b^2 - a^2, \\ \beta\gamma &= bc, \\ \alpha^2\delta^2 &= a^2d^2, \\ \gamma^2 - \delta^2 &= c^2 - d^2. \end{aligned} \right\}$$

Writing for a^2, b^2, c^2, d^2 , &c. a_1, b_1, c_1, d_1 , and squaring the second equation, we obtain a symmetrical system of equations, viz.

$$\left. \begin{aligned} \beta_1 - \alpha_1 &= b_1 - a_1, & \gamma_1 - \delta_1 &= c_1 - d_1, \\ \beta_1\gamma_1 &= b_1c_1, & \alpha_1\delta_1 &= a_1d_1, \end{aligned} \right\}$$

for determining $\alpha_1, \beta_1, \gamma_1, \delta_1$. Throwing out the solution $\alpha_1 = a_1, \beta_1 = b_1, \gamma_1 = c_1, \delta_1 = d_1$, only one other solution will be found to exist, which, restoring the original variables, becomes

$$\begin{aligned} \alpha^2 &= \frac{b^2 - a^2}{c^2 - d^2} d^2, & \beta^2 &= \frac{b^2 - a^2}{c^2 - d^2} c^2, \\ \gamma^2 &= \frac{d^2 - c^2}{a^2 - b^2} b^2, & \delta^2 &= \frac{d^2 - c^2}{a^2 - b^2} a^2, \end{aligned}$$

with the condition that $\beta\gamma = bc$.

The complete arithmetical determination of the signs to be given to the several quantities $\alpha, \beta, \gamma, \delta$ requires a distinct and detailed examination, which it would be irrelevant to enter upon in this place; it is enough to see that a second focus G at the distance $\frac{c}{b}$ from a given one F may be moved along the line FG to

a new focus H at the distance $\frac{\gamma}{\beta}$ from F, the *modulus* $\frac{b}{a}$ becoming

simultaneously replaced by $\frac{\beta}{\alpha}$, and the constant $\frac{d}{a}$ by the constant $\frac{\delta}{\alpha}$. I am not aware that M. Chasles has ever disclosed the

aperçu which led him to this unlooked for discovery. It is to be hoped that he will not allow future ages to labour under the same doubt as to the source from which he drew it as we must, it is to be feared, ever remain under with regard to the origin of Newton's rule, recently demonstrated, or Lambert's theorem, the motive to this paper. In this age of the world *euristic* is even more important to the promotion of science, and its secrets less likely to be recovered than those of mere *apodictic*.

Since a focus may be regarded as the intersection of two tangents at the circular points of infinity, we may generalize the problem of constructing the orbit by considering it as a particular case of constructing the conic which passes through two given points, touches two given straight lines, and has a principal axis of given length.

Taking the two given lines supposed to be inclined to each other at the angle α as the axes of coordinates, the equation to the curve may be written under the form

$$(Ax + Cy + 1)^2 = 2B^2xy,$$

which, writing

$$x = \xi - \frac{C}{2AC - B^2}, \quad y = \eta - \frac{A}{2AC - B^2},$$

becomes

$$A^2\xi^2 + 2(AC - B^2)\xi\eta + C\eta^2 = \frac{2B^2}{2AC - B^2}.$$

Adding $\lambda(\xi^2 - 2\cos\alpha\xi\eta + \eta^2)$ to the left-hand side of the equation, the discriminant of that side so augmented becomes

$$(\sin\alpha)^2\lambda^2 + (A^2 + 2\cos\alpha AC + C^2 - 2\cos\alpha B^2)\lambda + B^2(B^2 - 2AC).$$

Hence, calling the squared reciprocal of the given principal semiaxis

$$q, \text{ and writing } \lambda = \frac{2B^2}{2AC - B^2} q, \text{ we obtain}$$

$$4\sin\alpha^2 q^2 B^2 + 2(2AC - B^2)^2 (A^2 + 2\cos\alpha \cdot AC + C^2 - 2\cos\alpha \cdot B^2) q + (B^2 - 2AC)^3 = 0;$$

combining which with any of the four couples of linear equations,

$$pA \pm \sqrt{2pq}B + qC + 1 = 0, \quad p'A \pm \sqrt{2p'q'}B + q'C + 1 = 0,$$

obtained by substituting for x, y the coordinates of the two given points, we obtain six sets of quadruple solutions, making twenty-four *finite* solutions in all. This result is in perfect accordance with that which applies to the case of the tangents meeting at the focus; for when $\frac{1}{q}$ is the square of the principal semiaxis in which the focus lies, we have already found eight solutions; and when $\frac{1}{q}$ refers to the other semiaxis, we have

$$\frac{8}{q} = 8a(1 - e^2) = \frac{(s + s')^2(c^2 - \sigma^2)}{ss' + c^2 \pm \sqrt{(s^2 - c^2)(s'^2 - c^2)}};$$

which, considering c, s, σ given, leads to a biquadratic in s' which serves to fix the curve; and as there are four systems of values of s, σ arising from the permutations of the signs of ρ, ρ' , we thus have four times four, or sixteen solutions over and above the previous eight, making twenty-four in all, as in the general case.

We might generalize the problem otherwise by supposing given, not the magnitude of a principal axis, but that of a diameter through the intersection of the two given tangents; or, again, in quite a different direction by supposing three points P, Q, R to be given in a *Cartesian oval* defined by the equation $k\rho - \rho' = 2a$, ρ referring to a given focus F , and ρ' to a second focus G to be determined, a also being given, but k being to be determined. It is easy to see that in this case also the position of G may be obtained by the intersections of circles; for the ratios $PG : QG : RG$ will be known; there will thus be eight pairs of solutions arising from the permutations of the signs of ρ_1, ρ_2, ρ_3 which measure FP, FR, FR ; and calling $\frac{FG}{2a} e$, it would be an

interesting analytical question to express the eight systems of k and e in terms of ρ_1, ρ_2, ρ_3 , and c_1, c_2, c_3 the three chords joining P, Q, R ,—these six quantities, of course, being not independent but connected by the well-known equation between the mutual squared distances of any four points from one another on a plane.

Touching the Cartesian ovals, Mr. Crofton has well remarked that a circle may be regarded as one of a very peculiar kind. For if we take any two points electrical images of one another, inverses, in Dr. Hirst's language, or, as I prefer to call them, reciprocals or *harmonics* in respect to a given circle, the distances ρ, ρ' of any point in the circle from them will be connected by the equation $-k\rho + \rho' = 0$; so that any pair of harmonics whatever of a circle may be regarded as foci of such

curves. The third focus correlated to each pair will evidently be the centre ; for, calling its distance from any point in the circle ρ'' , we have $0 \cdot \rho + \rho'' = c$; in the first equation the modulus k is finite and the constant zero ; in the second the modulus is zero and the constant finite. Consequently a circle is a Cartesian oval, not only as a particular case of a conic, but *proprio Marte* and porismatically in quite another sort of way *. Now it is well known that a conic may be described by two forces (varying as the inverse square of the distance, and tending to its two foci). This led me to inquire whether some analogous theorem did not hold of a circle in respect to any of its pairs of foci, *i. e.* of harmonics ; and I find such is the case, as the annexed simple investigation will make manifest.

Call the radius of the circle *unity* ; $c, \frac{1}{c}$ the distances of two harmonics from its centre ; $\frac{\mu}{\rho^n}, \frac{\mu'}{\rho'^n}$ two forces tending to these points respectively ; then by duly assigning the initial velocity, we are at liberty to suppose the constant zero in the equation for *vis viva*, so as to be able to write

$$v^2 = \frac{2\mu}{(n-1)\rho^{n-1}} + \frac{2\mu'}{(n-1)\rho'^{n-1}} ;$$

we have also

$$\frac{\rho'}{\rho} = \frac{\frac{1}{c} - 1}{1 - c} = \frac{1}{c}.$$

In order that the circle may be described under the circumstances above supposed, it is necessary and sufficient that

$$v^2 = \frac{\mu}{\rho^n} \cos i + \frac{\mu'}{\rho'^n} \cos i',$$

* Thus it will be seen that, besides its derivation through the ellipse, the circle descends by a short cut immediately from the Cartesian oval ; recalling to mind the singular condition of consanguinity of the ill-fated descendants of Laius, at once children and grand-children to their mother, sons and brothers to their father. Viewed as sprung from the ellipse, there should be but two coincident Cartesian foci to the circle ; it is the fraternal aspect of the relationship which brings into view the existence of an infinite number of such foci in the circle ; every point in fact being a focus. This is explained by considering the circle so descended, not (like a conic) as a Cartesian oval with a branch at an infinite distance, but without such branch, and as doubled upon itself ; thus the circular points at infinity become each double points, and, as well remarked by Mr. Cayley, every line through either such double point is analytically a tangent to the curve, and thus every point in the plane of the circle, being the intersection of two such tangents, ought to be, as it is, a *focus*.

i, i' being the angles which ρ, ρ' respectively make with the normal; *i. e.*

$$\begin{aligned} v^2 &= \frac{\mu}{\rho^n} \left(\frac{1-c^2+\rho^2}{2\rho} \right) + \frac{\mu'}{\rho'^n} \left(\frac{1-\left(\frac{1}{c}\right)^2+\rho'^2}{2\rho'} \right) \\ &= \frac{1}{2} \left(\frac{\mu(1-c^2)}{\rho^{n+1}} + \frac{\mu' \left(1-\frac{1}{c^2}\right)}{\rho'^{n+1}} \right) + \frac{1}{2} \left(\frac{\mu}{\rho^{n-1}} + \frac{\mu'}{\rho'^{n-1}} \right). \end{aligned}$$

Hence, in order to satisfy this identity, we must have

$$\mu \frac{1-c^2}{\rho^{n+1}} + \mu' \frac{1-\frac{1}{c^2}}{\rho'^{n+1}} = 0,$$

or

$$\mu = \frac{1}{c^2} \left(\frac{\rho}{\rho'} \right)^{n+1}; \quad \mu' = c^{n-1} \cdot \mu;$$

so that

$$\frac{\mu'}{\rho'^{n-1}} = \frac{\mu}{\rho^{n-1}}.$$

And accordingly the required identity will be completely satisfied if we further make

$$\frac{\mu}{2} = \frac{2\mu}{n-1}, \quad \text{or } n=5,$$

which implies

$$\mu = c^4 \mu', \quad \text{or } \frac{\mu}{c^2} = \frac{\mu'}{\gamma^2},$$

c, γ being the distances of the respective centres of force from the centre of the orbit.

The *vis viva* consists of two equal parts, $\frac{\mu}{2\gamma^4}, \frac{\mu'}{2\gamma'^4}$, each centre contributing, as it were, equally to its production. To find the time, calling u the angle which the orbit swept out subtends at the centre, we have

$$\left(\frac{du}{dt} \right)^2 = \frac{\mu}{\rho^4},$$

or

$$t = \int \frac{du \rho^2}{\mu^{\frac{1}{2}}} = \frac{1}{\mu^{\frac{1}{2}}} \int du (1 + c^2 - 2c \cos u);$$

and P , the periodic time, will be $\frac{2F}{\mu^{\frac{1}{2}}} (1 + c^2)$, or, restoring the

value of a to the radius, the period becomes $\frac{2\pi}{\mu^{\frac{1}{2}}} a(a^2 + c^2)$, which of course is the same as $\frac{2\mu}{\mu'^{\frac{1}{2}}} a(a^2 + \gamma^2)$.

If we now suppose the two absolute forces μ , μ' , and δ the distance between them, to be given, the problem of determining the magnitude and position of the orbit and the periodic time may be easily effected; for we have only to find the equations of c , γ and a from the equations

$$c\gamma = a^2,$$

$$\frac{c^2}{\gamma^2} = \frac{\mu}{\mu'},$$

$$\gamma - c = \delta,$$

from which results

$$a = \frac{(\mu\mu')^{\frac{1}{4}}}{\mu^{\frac{1}{2}} - \mu'^{\frac{1}{2}}} \delta,$$

$$c = \frac{\mu}{\mu'^{\frac{1}{2}} - \mu^{\frac{1}{2}}}, \quad \gamma = \frac{\mu'^{\frac{1}{2}}}{\mu'^{\frac{1}{2}} - \mu^{\frac{1}{2}}},$$

$$P = 2\pi \frac{\mu^{\frac{1}{2}} \cdot \mu'^{\frac{3}{2}} + \mu'^{\frac{3}{2}} \cdot \mu^{\frac{1}{2}}}{(\mu'^{\frac{1}{2}} - \mu^{\frac{1}{2}})^3} \delta^3.$$

Also the velocity at either apse is given by the formula $v = \frac{\mu}{(a \mp c)^2}$, which gives $\frac{(\mu'^{\frac{1}{2}} \pm \mu^{\frac{1}{2}})^2}{\delta}$ for the two limiting velocities.

Again, the general expression for the time is

$$t = \frac{(a^2 + c^2)}{\mu^{\frac{1}{2}}} \left\{ u - \frac{2ac}{a^2 + c^2} \sin u \right\}.$$

Suppose, then, a planet to be describing an ellipse under the attraction of the sun, and a fictitious body moving in a circle described about its axis major to leave an apse simultaneously with the planet, and that its velocity parallel to the axis major always remains equal to that of the planet in the same direction. Then the arc swept out by such body subtends at the centre the angle which measures the eccentric anomaly of the planet, and may be termed its eccentric follower. The motion of this eccentric follower may be physically produced by supposing it to be attracted to two centres of force of proper absolute magnitudes and duly placed in the major axis, attracting according to the inverse fifth power of the distance; this is an immediate consequence from the preceding expression for t .

If we call M the absolute force of the sun, it will readily be seen that we must have

$$\mu = \frac{(a^2 + c^2)^2}{a} M,$$

$$\mu' = \frac{(a^2 + \gamma^2)}{a} M;$$

where c, γ are the distances of the two new centres of force from the centre of the planetary orbit, and satisfy the equation

$$\frac{2ac}{a^2 + c^2} = e,$$

or

$$c^2 - \frac{2a}{e} c + a^2 = 0,$$

which gives

$$c = \frac{a-b}{e}, \quad \gamma = \frac{a+b}{e};$$

b representing the semi-minor axis. c being equal to

$$\frac{a(1 - \sqrt{1-e^2})}{e}, \quad c - ae = a \frac{\sqrt{1-e^2}}{e} \{ \sqrt{1-e^2} - 1 \},$$

and is always negative, so that the interior centre of force always lies between the centre of the orbit and the sun; when e is small it lies about midway between these two points, but nearer to the latter than the former: *ex. gr.*, if we were to suppose

$e = \frac{3}{5}$, we should have $ae = \frac{3a}{5}$, $\frac{a(1 - \sqrt{1-e^2})}{e} = \frac{a}{3}$, which differs not very much from $\frac{3a}{10}$.

It is perhaps remarkable—at all events I am not aware whether any one has remarked, that the motion of the *eccentric follower* of a planet may also be brought about by a single force placed at the sun itself, attracting according to the law which is consistent with the body describing a circle. This is immediately apparent; for if we call S the position of the centre of force, C the centre of the circle, c the distance of S from C , a the radius of the circle, P any point in it, calling i the angle SPC , u the angle PCS , we have

$$v = \frac{h}{p} = \frac{h}{\rho \cos i} = \frac{h}{a - c \cos u};$$

so that $\frac{dt}{du} = \frac{a^2}{h} \left\{ 1 - \frac{c}{a} \cos u \right\}$, which proves the point in question*.

The force f for this case has been given by Newton in the third section of the *Principia*; it can be obtained instantaneously from the equation

$$v^2 = af \cos i = -\frac{a}{2} \frac{dv^2}{d\rho} \cdot \frac{\rho^2 + a^2 - c^2}{2\rho};$$

so that

$$\frac{dv^2}{d\rho \cdot v^2} = \frac{-4a\rho}{\rho^2 + a^2 - c^2},$$

or

$$v^2 = \frac{C}{(\rho^2 + a^2 - c^2)^2}; \quad f = -\frac{C\rho}{(\rho^2 + a^2 - c^2)^3}.$$

Calling ρ' the remainder of the chord R of which ρ is a part,

$$\rho^2 + (a^2 - c^2) = \rho^2 + \rho\rho' = \rho R;$$

so that f varies as

$$\frac{1}{\rho^2 R^3},$$

as given in the *Principia*, and of course, if the force-centre is at the extremity of a diameter, f varies $\frac{1}{\rho^5}$, which is the case in which our two reciprocal foci come together. When one of them is at the centre, the other goes off to infinity, and the actual amount of force exerted by it, $\frac{\mu'}{r'^5}$, or $\frac{\mu}{r^4} \cdot \frac{r'^4}{r'^5}$, becomes zero

when $\frac{\mu}{r^5}$ is finite; so that this case returns to that of a single force at the centre of the circle. If we wished to find the general law of the respective forces f, f' at the two reciprocal foci suitable to produce motion in the circle we might proceed as follows:—Calling i, i' the angles between the radii vectores drawn to these points from any point in the circle and the radius at that point, and writing

$$V = 2 \int dr \cdot f, \quad V' = 2 \int dr' \cdot f',$$

* Hence follows the *statical* proposition that the force which tending to any centre retains a point in a circular orbit may be resolved into two forces tending to two fixed centres, each varying as the inverse fifth power of the distance: this proposition will be generalized subsequently in the text.

we have to satisfy the equation

$$\frac{1}{2} \frac{dV}{dr} \cos i + \frac{1}{2} \frac{dV'}{dr'} \cos i' + V + V' = 0.$$

Writing $z = \frac{r}{\sqrt{c}} = \frac{r'}{\sqrt{r}}$, and taking $\psi(z)$, any arbitrary function of z , we may write

$$\frac{dV}{dr} + \frac{4r}{a^2 - c^2 + r^2} V = \frac{4r}{a^2 - c^2 + r^2} \psi(z),$$

or

$$\frac{dV}{dz} + \frac{z^2}{a^2 - c^2 + cz^2} V = \frac{4cz}{a^2 - c^2 + cz^2} \psi z;$$

and then

$$\frac{dV'}{dz} + \frac{\gamma z}{a^2 - \gamma^2 + \gamma z^2} V' = \frac{-4\gamma z}{a^2 + \gamma^2 - \gamma z^2} \psi(z).$$

Integrating these equations, we find

$$V = \frac{4c}{(a^2 - c^2 + cz^2)^2} \int dz (z(a^2 - c^2 + cz^2)) \psi z,$$

$$V' = \frac{-4\gamma}{(a^2 - \gamma^2 + \gamma z^2)^2} \int dz (z(a^2 - \gamma^2 + \gamma z^2)) \psi z;$$

also

$$f = \frac{\sqrt{cz}}{a^2 - c^2 + cz^2} (\psi z - V).$$

Hence, making

$$\psi z = \frac{c^{\frac{3}{2}} \phi' z}{2z(a^2 - c^2 + cz^2)},$$

we have

$$f = \frac{\phi'(z)}{(\gamma - c + z^2)^2} - \frac{4z\phi z}{(\gamma - c + z^2)^3},$$

$$f' = -\frac{\phi' z}{(c - \gamma + z^2)^2} + \frac{4z\phi z}{(c - \gamma + z^2)^3},$$

ϕ being any arbitrary function, and z representing $\frac{\gamma}{\sqrt{c}}$ or $\frac{\gamma'}{\sqrt{\gamma}}$.

A similar method will apply to the determination of the forces at the foci whereby any conic may be described*.

* Employing the equation

$$V + V' + \frac{1}{2} \left(\frac{dV}{dr} + \frac{dV'}{dr'} \right) \rho \cos i = 0,$$

replacing $\rho \cos i$ by its equivalent $\frac{rr'}{a}$, writing $r - a = a - r' = z$, and decom-

It may be worth while pointing out a somewhat singular consequence of the laws that have been above established for the motion of a body in a circle about two reciprocal points as centres of force. It is an immediate and now well known, although for a time singularly overlooked, consequence* of the linear form of the equation $(\Sigma f \cos i)\rho = C - 2\Sigma \int dr(f)$ [where f is any central force, and i the angle which it makes with ρ , the radius of curvature at any point], which equation† exhibits the

posing the equation above written into

$$V + \frac{1}{2} \frac{dV}{dz} (a^2 - z^2) = \phi z; \quad V' + \frac{1}{2} \frac{dV'}{dz} (a^2 - z^2) = \phi z,$$

integrating the two equations, and making suitable substitutions, thence results

$$\left. \begin{aligned} f &= \frac{r'}{r} \psi'(r-a) - 2\mu a \frac{\psi(r-a)}{r^2}, \\ f' &= \frac{r}{r'} \psi'(a-r') - 2\mu' a \frac{\psi(a-r')}{r'^2}. \end{aligned} \right\}$$

* See an article by M. Serret among the valuable notes of M. Bertrand's edition of the *Mécanique Analytique*. The principle referred to must be taken with *analytical* latitude, or the range of its application will be unduly restricted. For instance, it is well known and easily demonstrable that a body starting from rest in a position where it is equally drawn by two forces converging to centres attracting according to the law of nature, will oscillate in the arc of an hyperbola. Here the principle seems inapplicable; for the hyperbola will be concave to one focus of attraction and convex to the other, but a curve actually described about either focus would be concave towards it. But in fact the principle does apply; for, analytically speaking, *any conic whatever* may be described about an attractive centre of force varying as the inverse square; only if it be convex to the centre of attraction, its *vis viva* will be a negative quantity, and the motion imaginary. In the case above supposed, the *vis viva* due to each centre of force ending singly will be equal, but with contrary signs, so that the body in such position must be supposed to be at rest; then, by virtue of the principle enunciated, it will for ever continue to move in the hyperbola, in which it would move *really* under the influence of one centre, *imaginarily* under that of the other,—the imaginary motion blended with the real continuous one changing the character of the latter into a reciprocating movement, which is in no way contradictory to M. Serret's theorem, which only determines the *locus*, but not the *direction* of the movement at any point.

† I am informed by the highest authority, the author of Reports on Mechanics, which have become classic, that he has never seen this equation anywhere before employed. It is of course an obvious generalization of Newton's rule, connecting the velocity with that due to a single central force acting through one-fourth of the chord of curvature. As it springs from a combination of the law of *vis viva* with that for centrifugal force, I propose to call it the *Equation of Radial Work*. By aid of it, it is easy to establish the following theorem, giving the most general binary system of forces acting to *two centres*, which will make a body describe *any* given orbit. Call V, V' the respective *forcefunctions* (so that $\frac{dV}{dr}, \frac{dV'}{dr'}$ are the

sole necessary and sufficient condition for any determinate orbit being described, that, if several sets of forces taken separately can make a body describe a certain path, then all the sets acting collectively will make it describe the same path, provided the *vis viva* at the starting-point, on the latter supposition, is the sum of the *vires vivæ* on the former one.

Suppose, now, a zone of matter bounded by two arbitrary contours P, Q to lie anywhere within a circle C, and another zone bounded by two contours P', Q', the geometrical inverses (or reciprocals) of P, Q to lie outside the same. Then these two zones may be divided into corresponding rectangular elements by transversals drawn through the centre of the circle, points being taken all along every radius of one zone, and corresponding points along the radii of the other. If r, r' be the distances of the centres of any two corresponding elements thus obtained from the centre of the circle, $d\theta$ the angle between the two transversals which pass through both pairs of points in both figures E, E', the areas of the respective elements will be

$$d\theta \cdot dr \cdot r; d\theta(-dr')r', \text{ i. e. } -d\theta d\left(\frac{a^2}{r}\right)\frac{a^2}{r};$$

so that

$$\frac{E}{E'} = \frac{r^4}{a^4} = \frac{r^2}{r'^2}.$$

Hence if the densities of E, E' be the same, and they *attract* with forces varying as the inverse fifth power of the distance, they will serve to make a body describe the circle in question, E, E' taking the place of μ, μ' and r, r' of c, γ in our previous formulæ; and as this is true of each pair of elements, it will be true of the two entire zones which they compose, the law of density being perfectly arbitrary, except that it must be the same for *corresponding points* in the interior and exterior zone. The contour Q may be made to coincide with Q' at the circumference of C if we please; and then, as a particular case of the proposition above, we may suppose the united zones to consist of homoge-

two central forces). Call P, P' the squared perpendiculars on the tangent from the two centres respectively, ϕ an *arbitrary* function of any *affection* of the position of the revolving body (*ex. gr.* of the length of arc or radius of curvature at any point), then

$$V = \frac{\int \phi dP}{P}, \quad V' = \frac{-\int \phi dP'}{P'}$$

will be the general system in question. When the stored-up work for each point in the orbit is known, the *radial equation* gives the central forces without integration. Thus, *ex. gr.*, if a body move in an ellipse with uniform velocity acted on by forces towards the foci, the equation in question shows that they are equal, and vary as the inverse square of the conjugate diameter.

neous matter, or, if we please, of matter whose density at any point is only a function of the angular position of the line joining the point to the centre of the circle. Thus, if we suppose a plate of matter of uniform density and of indefinite extent, and attracting according to the law of the inverse fifth power, a point anywhere placed upon it may be made to move in any desired circle under the influence of the plate's attraction, if we cut away a portion of the plate surrounding the centre of such circle, and leave a proper margin exterior to the circle—the rule being that the intrados of the figure so obtained may be of any form whatever, provided the extrados be its electrical image or inverse. The initial velocity to be communicated to the moving point will of course be determined by the form of either of these bounding curves.

It is hardly necessary to add that instead of a zone we may take a patch of matter bounded by a contour of any form within the circle C, and then, finding the inverse of this contour so as to obtain a corresponding external patch, the two together, by the combined attractions of their particles according to the inverse fifth power of the distance, will serve to make a body describe the circle C; and conversely, since any two circles may be made reciprocals (inverses) to each other by duly determining the centre and radius of the circle of reference, it follows that any two circles of matter attracting according to the above law, will serve to keep a body moving in a certain third circle.

By calculating the attractions of these two circular images, and replacing them by forces tending to their centres, we shall be able to transform and generalize the results previously obtained. But first it will be expedient to recall attention to the form of the single central force which serves to make a body describe a circle. We have found that such force, when the centre lies

within the orbit, is of the form $\frac{\mu\rho}{(\rho^2+k)^3}$; and it is easy to see

that when external thereto, it takes the form $\frac{\mu\rho}{(\rho^2-k)^3}$,—in either case k being the product of the two distances of the force-centre from the extremities of the diameter drawn through it; when the force is external, this product is the square of the tangent drawn to the circle from the centre. At the points of contact the force and velocity both become infinite, and the latter changes its sign.

In a physical sense, only the concave part of the circle will be described by virtue of attraction to the centre, the revolving body going off in a straight line towards the centre when any point of contact is reached, and in like manner only the convex part by virtue of the repulsive force from the centre, the body

going off in a straight line towards infinity on reaching such point; but inasmuch as in either case an infinitesimal deviation from the tangential direction will cause the remainder of the orbit to be described, we may consider, in an *analytical* sense, that the revolving body under the influences of such force describes the entire orbit. We may give the name of *cyclogenous*

force to any central force of the form $\frac{-\mu\rho}{(\rho^2 \pm k)^3}$, and, if we care to draw the distinction, call it internally cyclogenous or endocyclogenous when the k is positive, and externally cyclogenous or exocyclogenous when k is negative. If we call the cyclogenous-force-function V , so that $\frac{dV}{d\rho}$ is the cyclogenous force itself, we

have, by integration, $V = \frac{1}{4} \cdot \frac{\mu}{(\rho^2 \pm k)^2}$.

Let us now proceed to calculate the attraction of a circular plate (of radius r) of uniform density, whose particles attract according to the law of the inverse fifth power of the distance upon an external particle at the distance ρ from the centre. If

we call this $g \frac{dP}{d\rho}$, we have

$$P = \frac{\mu}{4} \int_0^r dr \int_0^{2\pi} \frac{r dr \cdot d\theta}{(r^2 + \rho^2 - 2r\rho \cos \theta)^2}.$$

By comparison of $\int_0^{2\pi} \frac{d\theta}{(r^2 + \rho^2 - 2r\rho \cos \theta)^2}$ with the integral which represents twice the area of an ellipse of excentricity,

$\frac{2r\rho}{r^2 + \rho^2}$, we find instantaneously

$$P = \frac{\pi\mu}{4} \int_0^r \frac{2(\rho^2 + r^2) dr}{(\rho^2 - r^2)^3} = \frac{\pi}{4} \frac{gr^2}{(\rho^2 - r^2)^2}.$$

Thus P is of the form of the cyclogenous-force-function, so that the *force of attraction to the centre of a circular plate attracting according to the inverse fifth power of the distance, upon an exterior point, is an external cyclogenous force*. From this we may easily draw the conclusion that any circular orbit cutting orthogonally a circular plate whose particles attract according to the inverse fifth power of the distance may be described (or, at all events, the concave part of it be described) by virtue of such force of attraction.

Let us now consider the joint effect of two such circular plates, *images* of one another, lying one entirely within, the other entirely without a given circle. The centres of two such circles, it will be borne in mind, are *not* images of one another. Let r, r' be the radii of the two images, a of the image

circle; call the distances of the centres of the images from that of the image circle c, c' respectively. The points of contact of the images with a common *exterior* tangent will be corresponding points, and this tangent will pass through the centre of the circle of reference; whence we easily derive $(c^2 - r^2) \cdot (c'^2 - r'^2) = a^4$, and by similar triangles,

$$\frac{c}{c'} = \frac{r}{r'}.$$

Hence

$$(c^2 - r^2)^2 = \frac{c^2}{c'^2} a^4.$$

Whence, remembering that r must be less than c , we have

$$c^2 - r^2 = \frac{c}{c'} a^2, \quad c^2 - r'^2 = \frac{c'}{c} a^2;$$

so that

$$r^2 = c^2 \left(1 - \frac{a^2}{cc'}\right); \quad r'^2 = c'^2 \left(1 - \frac{a^2}{cc'}\right)^*.$$

Consequently, calling $1 - \frac{a^2}{cc'} = \pm q$, if F, G , two points in the diameter of the image circle, be distant c, c' respectively from its centre, and two cyclogenous forces $\frac{\lambda c^2 \rho}{(\rho^2 \mp qc^2)^3}, \frac{\lambda c'^2 \rho'}{(\rho'^2 \mp qc'^2)^3}$ tend to F and G , two such forces will serve to make a body describe a circle, and, as we shall see, will be statically equivalent to a single cyclogenous force tending to a fixed point, presently to be determined†.

It follows from what has been shown of any two corresponding elements in the two figures, that the total *vis viva* contributed by each at any moment of time, to the entire amount of stand-up work in the revolving body is the same; consequently, confining our attention to one of the image circles, we see that

$v^2 \propto \frac{1}{(\rho^2 \pm qc^2)^2}$. Hence using u to denote, as before, the angle at the centre, we have

$$\frac{du}{dt} \propto a^2 + c^2 \pm qc^2 - 2ac \cos u,$$

which is of the *form* which gives the motion of a planet in eccentric anomaly; consequently, by a proper adjustment of the constants, the motion due to the cyclogenous centres F, G may

* Calling F, G the two centres, F', G' the images of F, G respectively, O the centre of the image circle, it is easily seen that $r^2 = FO \cdot FG'$, $r'^2 = GO \cdot GF'$.

† The proof of this through the medium of the two circular images requires $-q$ to be employed; but the laws of *analytical* continuity allow q to be made to change its sign.

be made identical with the motion in a circle of radius a with centre at O, about the single cyclogenous-force centre at S. Call ae the distance of S from O, M the absolute force at S, then, comparing the *vis viva* on the two suppositions at the two apsidal points, and again availing ourselves of the law of equal production of *vis viva* from the two force-centres F, G, we obtain

$$\frac{2\lambda c^2}{((a \mp c)^2 + qc^2)^2} = \frac{M}{((a \mp ac)^2 + (a^2 - a^2 e^2))^2}.$$

Hence
$$\frac{(a-c)^2 + qc^2}{(a+c)^2 + qc^2} = \frac{a-ae}{a+ae}$$

or
$$(1+q)c^2 - \frac{2a}{e}c + a^2 = 0.$$

Calling c, c' the two roots of this equation, we have

$$\frac{1}{c} + \frac{1}{c'} = \frac{1}{2ae};$$

or, which is the same thing, the points O, F, S, G form a system of four points in harmonic relation.

Hence, if we take a system of points, F, F', F'' . . . G, G', G'' . . . in *involution*, the double points of the system being at S and O, the cyclogenous force at S will be statically equivalent to two cyclogenous forces directed to any two corresponding points F, G.

It is possible that this theorem may be modified so as to admit of further generalization, and be made to extend to an arbitrary system of points in involution, without regard to the condition of O being one of the double points; but I have not had time to consider this point.

In the particular case where F, G become images, in respect to the circular orbit annexed to the force at S, the cyclogenous centres F, G become centres of attraction, following the law of the simple inverse fifth power, as already found. Since in all cases the absolute forces at F, G are proportional to the squares of their distances from O, if we make $cc' = a'^2$, and draw the circle whose centre is at O and radius is a' , and take two figures, images of one another in respect to this circle, by the same reasoning as applied to the case of $a' = a$ it may be proved that, provided the densities at corresponding points of such images be the same, and the particles attract according to a certain fixed cyclogenous law, their joint action will support a body in a circular orbit whose radius is a and centre at O. We might again assume two such images to be circular, calculate the law of attraction towards the centres according to the supposed law, and so return to a new system of conjugate points replacing F and G; but I have not had time to ascertain whether such

transformation would or would not lead to a new theorem, or merely, as is possible, to a repetition, with a new set of constants, of the one already obtained.

It is hardly necessary to point out how strongly the analogies established in the preceding investigations point to the existence of some simple dynamical theory of the Cartesian ovals under the attraction of forces directed to their foci. The investigation of such theory cannot but tend materially to the elucidation of the essential properties of these most interesting and as yet little-understood curves, the natural parents of the conic sections viewed as focal curves.

In conclusion it may be observed that, in the foregoing paper, it has been seen how a single orbital force passing through a fixed centre may be resolved into others of a more simple form. This suggests a more general subject of investigation, where the force to be so resolved, instead of passing through a fixed point, is tangential, or, better, normal to a fixed curve or surface.

Such an inquiry by no means belongs to ideal mechanics; for it would correspond to the case of the motion under the earth's attraction of a body near the earth's surface, considered as a surface of fluid equilibrium.

K, Woolwich Common.

20th December 1865.

VIII. *Notices respecting New Books.*

The Mathematical Writings of D. F. Gregory, M.A., late Fellow of Trinity College, Cambridge. Edited by W. WALTON, M.A. Cambridge: Deighton, Bell, and Co. 1865.

IT is very difficult for the outside world to do justice to the writings of a man removed by an early death. It is not that what he has produced is intrinsically unimportant (it may or may not be so), but that even when it is important it is still altogether disproportioned to the power to which it owes its origin, and consequently is inadequate to sustain the reputation in which the writer was held by the small circle of friends to whom he was intimately known. And this is true in the present case, although some of the articles which compose the 'Mathematical Writings' of Mr. Gregory evince great originality, and at the time of their publication powerfully contributed to the advancement of mathematical science. Still it is very desirable that the original memoirs of a man like Mr. Gregory should be brought together and competently edited, both as a memorial of one who ought not soon to be forgotten, and as being in themselves interesting and suggestive articles, and in some cases of first-rate importance.

Mr. Gregory was the youngest son of Dr. James Gregory, the distinguished professor of medicine in the University of Edinburgh. He was born in April 1813, and died in February 1844, having not

quite completed his thirty-first year. Previously to entering at Trinity College, Cambridge, he had attended classes at the University of Edinburgh, where he was a favourite pupil of Professor Wallace. He passed the examination for the degree of B.A. at Cambridge in 1837, and was elected Fellow of Trinity College in 1840. In consequence of illness he left Cambridge in the spring of 1843, and never returned. In the brief interval between the first and last of these dates he was actively employed in promoting the mathematical studies of the University of Cambridge. This he did in many ways—partly by lecturing and examining, but chiefly by his writings. He was mainly instrumental in establishing the Cambridge Mathematical Journal, and, excepting a short interval, was its editor from the time of its first appearance till a few months before his death. He contributed largely to the Journal; in fact nearly half the first volume came from his pen, as also did a considerable part of both the second and third volumes. These contributions, in addition to a paper read before the Royal Society of Edinburgh, compose the present volume. The most important are those in which he works out the relations existing between the symbols of operation and those of quantity, and applies his results to the solution of large classes of differential equations. These papers are undoubtedly of very great value, and will be read with pleasure by those who take an historical interest in mathematics. Another class of papers relates to the interpretation of results in symbolical algebra, such as that “On the Impossible Logarithms of Quantities,” “On the existence of Branches of Curves in several Planes,” &c. Other papers are on detached subjects—“Demonstrations of certain properties of Triangles,” “Solutions of some Problems in Transversals,” &c. The volume contains thirty-seven papers in all: of these but three have any reference to physics; viz. one, “On the Sympathy of Pendulums,” and another “On the Motion of a Pendulum whose point of Suspension is disturbed,” and a “Note on a Problem in Dynamics.” The two former Mr. Gregory wrote in conjunction with Mr. Archibald Smith.

It is well known that, besides these articles, Mr. Gregory wrote a collection of “Examples of the Processes of the Differential and Integral Calculus.” It may be added that in this work he embodies the results of his most important papers. At the time of his death he left an unfinished MS. Treatise on Geometry of Three dimensions, which was completed and published by Mr. Walton in 1845.

The present volume is in some sort a companion volume to the ‘Mathematical and other writings of Robert Leslie Ellis,’ published about two years ago, and which we noticed at the time. As in the case of that volume, so of this, Mr. Walton has shown himself a faithful editor. The printing and the general appearance of the volume leave nothing to be desired. Mr. Ellis was an intimate friend of Mr. Gregory, and succeeded him as editor of the Mathematical Journal. On his death Mr. Ellis wrote a brief life of him, which appeared in the Mathematical Journal, and is reprinted here. Perhaps, as we are noticing a memorial volume, we may be allowed to make from it the following extract. Mr. Gregory had been aware for a short time before his death “that the end was at

hand; and, with an unclouded mind, he prepared himself calmly and humbly for the great change, receiving and giving comfort and support from the thankful hope that the close of his suffering life here was to be the beginning of an endless existence of rest and happiness in another world. He retained to the last, when he knew that his own connexion with earthly things was soon to cease, the unselfish interest which he had ever felt in the pursuits and happiness of those he loved. A few words may be allowed about a character where rare and sterling qualities were combined. His upright, sincere, and honourable nature secured to him general respect. By his intimate friends he was admired for the extent and variety of his information, always communicated readily but without a thought of display,—for his refinement and delicacy of taste and feeling,—for his conversational powers and playful wit; and he was beloved by them for his generous, amiable disposition, his active and disinterested kindness, and steady affection. And in this manner his high-toned character acquired a moral influence over his contemporaries and juniors, in a degree remarkable in one so early removed.”

IX. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxx. p. 378.]

October 30, 1865.

THE following officers were elected :—

<i>President</i>	Rev. W. H. Cookson, D.D.
<i>Vice-Presidents.</i> {	Mr. I. Todhunter.
	Dr. Paget.
	Professor Challis.
<i>Treasurer</i>	Rev. W. M. Campion.
<i>Secretaries</i> .. {	Professor Cardale Babington.
	Professor Liveing.
	Rev. T. G. Bonney.
<i>New Members</i> {	Rev. W. G. Clark.
<i>of the Council.</i> {	Mr. R. Potter.
	Rev. N. M. Ferrers.

The following communications were made to the Society :—

By Mr. A. R. Catton—

1. “On the Synthesis of Formic Acid.”
2. “On the possibility of accounting for the double refraction of Light by the vibrations of a *continuous* elastic medium kept in a state of constraint by the action of the material molecules.”

By Professor Cayley—

3. “A new Theorem on the Equilibrium of four Forces acting on a solid Body.”

Defining the “moment of two lines” as the product of the shortest distance of the two lines into the sine of their inclination, then, if four forces acting along the lines 1, 2, 3, 4 respectively are in equilibrium, the lines must, as is known (Möbius), be four generating lines of an hyperboloid; and if 12 denote the moment of the lines

1 and 2, and similarly 13 the moment of the lines 1 and 3, &c., the forces are as

$$\sqrt{23.34.42} : \sqrt{34.41.13} : \sqrt{41.12.24} : \sqrt{12.23.31}.$$

Calling the four forces P_1, P_2, P_3, P_4 , it follows as a corollary that we have

$$P_1 P_2 . 12 = 12 . 34 \sqrt{13.42} . \sqrt{14.23} = P_3 P_4 . 34 ;$$

viz. the product of any two of the forces into the moment of the lines along which they act is equal to the product of the other two forces into the moment of the lines along which they act,—which is equivalent to Chasles's theorem, that, representing a force by a finite line of proportional magnitude, then in whatever way a system of forces is resolved into two forces, the volume of the tetrahedron formed by joining the extremities of the two representative lines is constant.

November 13.—By Professor Sedgwick, F.R.S., “A Sketch of the Geology of the Valley of Dent, with some account of a destructive Avalanche which fell in the year 1752.”

The valley of Dent lies in the north-west corner of Yorkshire, which is thrust in between Westmoreland and Lancashire, beyond the natural limits of the county. The upper part of the valley is excavated in the carboniferous groups which are continued southwards into Nottinghamshire, and northwards into Durham and Northumberland, and through the greatest part of their range form the watershed between the east and west coasts of England. All the valleys that drain down to the land are partly formed in rocks of the carboniferous age. In the upper part of Dent Dale, which is one of these tributaries, the great scar-limestone appears only near the bottom of the valley, while the sides are formed of soft shale alternating with harder bands of sandstone and limestone; and the whole series is capped by mill-stone grit. The rainfall in some portions of the Lake mountains is not less than 150 or 160 inches in the year. Among the neighbouring carboniferous mountains the rainfall is much less; but still it is at least three times the English average; and the winter fall of snow is in some years enormous. Hence the *becks*, or mountain-streams, are often greatly swollen, and the *gills*, or lateral branches, frequently descend in brawling torrents from the mountain-side into the lower valley through deep ravines and lateral valleys that have been excavated out of the shales and sandstones in the course of past ages. On rare occasions a great fall of snow, accompanied by a violent wind, will almost fill up the ravines and lateral valleys, and form a dam across the descending water; and should there be a sudden thaw afterwards, the descending gills may be held up for a while till the pressure of the water drives down the barrier, and an avalanche is formed of mingled snow and water (provincially called *a brack*), which rushes down with the roar of thunder, and bears all before it into the *beck* below. On the 6th of February, 1752, a very large one fell, destroying several houses and farm buildings, and killing seven people, besides several head of cattle. The following letter, written by an eye-witness, describes

the catastrophe (the spelling and punctuation have been slightly modernized):—

“Harbourgill, 6th of the 2nd month, 1752.

“Dear Bro’ and Sister,—

“These few lines I hope will find excuse: for it’s not without a cause that I have written no sooner to you. I fully purposed to have seen you a considerable time since: but now, as things are at present, I have lost all hopes of coming. Yet through the good providence of Heaven we are all alive and pretty well in health: which is more than could be well expected, considering what dismal times it has been with us in Dent. I hope I shall never live to see the like again: for we had the greatest storm of wind and snow that continued for above a week with very little intermission: so that all the watercourses, both in the mountains and elsewhere, were made level; the like never being remembered, for it excited the curiosity of several persons to view them with wonder and astonishment: yet little thinking that the consequences would have been so tragical to many. For at the breaking up of the storm (*i. e.* frost) it began to rain exceedingly in the evening, which continued all night and the next day to that degree that, by 11 o’clock, the dismal scene began. For the snow in the watercourses being no longer able to sustain the great quantities of water, all began to slide down the mountains together with incredible swiftness, driving great rocks, stones, and earth, all before it; roaring like claps of thunder; which made us run out of doors to see what was coming upon us. We ran to look at the Gill; and we directed our sights (by the noise that it made) the right way; and the frightfulness of the appearance at the very first sight, which was when about the middle of the pasture, made us run for our lives; and we got no further than from the yet (*i. e.* gate) to the sycamore trees, before the stable, peat-house, and all the calf-parrack (*i. e.* paddock) and cow-parrack, was in a heap of the most shocking ruins that ever your eyes beheld. I believe from the first sight of it, when it was coming, till all was overturned, was less than the quarter of a minute’s time. It has brought rocks down past the middle of the house, which had gone through the peat-house and stable, that I think three or four yoke of oxen could not be able to move. The poor old horse was crushed to pieces in a moment. Nothing but the good providence of God has preserved us from perishing; for it’s amazing to think how the barn stood the violence of the shock. The waters run round our dwelling house, broke down the garden wall, and continued running through it till next day in the morning; so that it’s become a bed of sand. It was about 11 o’clock when this happened, and we went from place to place, not knowing where to be safe, expecting every moment more of the like nature; which accordingly happened; for I think in the space of two hours the face of things was so changed that one scarcely could have known them. For they came down almost every slack*, carrying all the walls before them; so that we were obliged to run from one place to another to escape their fury, which was with difficulty: for it continued raining extremely, that we were wet to the naked skin,

* Slack (*coom* or hollow in the hill-side).

not daring to come in any house. And it drawing towards night, we resolved to make an attempt to get to brother John's, and accordingly set forwards, and got up at our pasture head on to the moor, and with difficulty got over Harbourgill, and so forwards to the Munkeybeck. But we knew that the bridge was broke down, so that we must be obliged to pass it somewhere on the moor, and we waded through the water and snow till we were almost spent in extreme wet and fatigue; and at last got over a little below where our peat-fell is (tho' with very great hazard of our lives), at last, my poor old Father and Betty being almost quite spent, he having only one shoe on one foot the greatest part of that time. Then when we were got over, it gave us some fresh encouragements, and we arrived at Bro' Johns just before it was dark, where we were thankful to see the faces of one another in a place of more safety. We went three nights successively to Bro' John's to lodge, not daring to stay about the old place. Old Francis Swinbank [*rect.* Swithinbank] and Thomas Stockdale's whole family perished in a moment about the same time that the thing happened with us, being seven in number. Likewise John Burton, Stone House, had a barn swept away and a cow killed.

"I hope these few broken hints will be excused, for I am not very good at writing at this time, all being so in confusion. Sr. greatly desires you would come to see us as soon as well can. For our love is very much towards you. You perhaps may think I have outstretched, but if you please to come your eyes will convince you to the contrary. For I have not told you one half. So shall conclude your very loving Brother,

"THOMAS THISTLETHWAITE."

"Betty's kind love is to you both, but Sr. in particular."

Mr. J. W. Clark, Trinity College, read a paper upon the Rib of a Whale found by some fishermen near Sherringham (four miles N.W. of Cromer). It was discovered after a high tide, which caused a fall of the cliff; it was reported to have been imbedded in drift gravel. Mr. Clark stated he had compared it with the rib of *Physalus*, with which it did not agree; for the tubercle and head of the rib were very much wider apart in the fossil specimen than in *Physalus*. It resembled *Balænoptera* more nearly, and still more closely *Balæna mysticetus*. It was probably the fourth or fifth rib of the left side. He remarked that a few months since some cervical vertebræ had been found at Plymouth which probably belonged to *Balæna Biscayensis*, a whale which was nearly extirpated in the sixteenth century. It was, however, still occasionally met with, a cow and cub being not long since seen near Pampeluna, and the latter captured. He could not positively refer the rib exhibited to any known species.

Professor Sedgwick remarked that large cetacean vertebræ had been found at Landbeach, Cambridgeshire, and in the Crag of Norfolk and Suffolk. This was the largest rib that he had ever seen; but he had great doubt, from its general appearance, whether it could have come out of the gravel.

Mr. Newton remarked that our knowledge of whales had of late

been greatly enlarged; for of the bodies which had been stranded so many new species and even new forms had been observed, that hardly two had been found to be identical.

Professor Miller exhibited two new forms of heliotrope, explaining at the same time the difficulties in signalling, which they were intended to overcome, commenting upon the relative merits of those invented by Gauss, Steinheil, and others, and explaining the special advantages of the two which he exhibited.

Professor Miller also communicated a Supplement to the Crystallographic method of Grassmann.

Mr. G. F. Browne, St. Catherine's College, communicated some Notes upon some Ice-caves explored during the summer of 1865. Two of these he had visited during the previous summer, and he found that there was a somewhat greater quantity of snow in the caverns than there had formerly been. In the first cave he again examined a pit in the ice about 70 feet deep, but, owing to the dangerous condition of the ice, was unable to descend into it. In the second cave he had again cut through a curtain of ice into an icy tunnel; but this year the diameter of the tunnel was so much smaller that he was unable to descend it, although provided with ropes for the purpose. He described some flies found inside the tunnel. The third cave had not previously been explored; it was an oval in shape, with a level floor of ice. He had descended for about 12 feet between the ice and the rock, and there found a narrow tunnel which appeared to lead to a subglacial reservoir containing water. He ascertained that the ice was at least 24 feet thick; but it was impossible to descend the tunnel.

Mr. Bonney, who had accompanied Mr. Browne, made some observations on the general character of the country, expressing his opinion that the glaciers were formed by the accumulation of snow in suitable fissures; and remarking that the prismatic structure of the ice noticed last year by Mr. Browne was very conspicuous.

X. Intelligence and Miscellaneous Articles.

ON THE DENSITY OF OZONE. BY M. SORÉT.

(Abstract of a letter to Professor Tyndall.)

I OUGHT to have written to you sooner, but I have been much engaged in trying to finish part of my investigation on the density of ozone, of which I have spoken to you. You are aware of the difficulties of the question, and of the impossibility of determining this density by weighing; for, on the one hand, ozone is never obtained quite pure, and, on the other, bodies like iodide of potassium do not absorb ozone, but merely part of its constituent atoms, without change of volume. You are familiar with the hypothesis which I have suggested, which is analogous to that of Weltzien, adopted by Clausius; it consists in considering ordinary oxygen as consisting of two atoms OO , and ozone of three OO, O . On this hypothesis the density of ozone ought to be one and a half times as much as that of ordinary oxygen.

I have therefore endeavoured to arrive at this value, and I believe

I have succeeded. I first made some attempts by diffusion, but hitherto without any great success; but I expect an apparatus which I have had constructed, and with which I hope to succeed. I then sought for some substance which should really absorb ozone, but without decomposing it as iodide of potassium does. I have found it in oil of turpentine and in oil of cinnamon, which destroy ozone in a mixture of this gas and of oxygen, producing a diminution of volume. It is natural therefore to suppose that these oils absorb the ozone entire; hence the possibility of determining the density, not perhaps with great accuracy, but with a sufficiently close approximation to decide whether it is $1\frac{1}{2}$, 2, or 3 times that of oxygen. It is sufficient to compare the diminution in volume of the gas treated with the oil, with the volume of oxygen absorbed by the iodide of potassium, or, what comes to the same thing, with the increase of volume which it experiences under the influence of heat. I have thus found that the diminution of volume by the oil is the double of the increase of volume by heat; and I thence conclude that *the density of oxygen is once and a half that of oxygen*, and have thus confirmed the hypothesis mentioned above. The following are the results which I have obtained:—

Absorbent.	Diminution in volume.	Expansion by heat.		
		Calculated.	Observed.	Difference.
	cub. centims.	cub. centims.	cub. centims.	cub. centims.
Oil of turpentine	6·8	3·4	3·77	+0·37
" "	5·7	2·85	3·20	+0·35
" cinnamon.....	5·8	2·90	3·14	+0·24
" turpentine	5·6	2·80	3·32	+0·52
" "	6·7	3·35	3·30	—0·05
" cinnamon.....	6·9	3·45	3·45	
" "	5·7	2·85	2·72	—0·13

The agreement is as close as can be expected in this kind of measurement, especially for the three last experiments, in which I avoided a slight source of error which I had not at first perceived. I worked with flasks of about 230 cubic centims. capacity, and with oxygen ozonized by electrolysis.

NOTE OF AN EXPERIMENT ON VOLTAIC CONDUCTION.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In the last Number of the Philosophical Magazine I suggested a crucial experiment to decide whether the electric force of the voltaic pile is conveyed by the entire thickness of the conductor or by the external surface only. It has since occurred to me that the point might perhaps be tested without delicate apparatus, by an appeal to the sense of touch. I provided two polished steel cylinders exactly alike, each 3 inches long and $\frac{1}{10}$ inch diameter. One I covered with bright copper wire $\frac{1}{100}$ inch diameter, coiled round it quite tight and close, thus forming a cylinder 0·12 inch diameter, having a copper surface. The other I covered with steel wire $\frac{1}{50}$ inch diameter, coiled also close together, thus forming a cylinder 0·14 inch dia-

meter, having a steel surface. These two cylinders were connected end to end by a thin copper wire, the interval bridged over by this wire being about $\frac{1}{4}$ inch. The other ends of the cylinders were similarly connected to the polar terminals of a Bunsen cell half charged (8 water-sulphuric acid; commercial nitric acid; amalgamated zinc cylinder 6 inches by 3 inches). These terminals were made from a rope of four No. 16 copper wires twisted together; each was about 18 inches long. After waiting two or three minutes, the steel-covered cylinder was found to be much hotter than the copper covered. This was quite sensible to the fingers; but the most satisfactory way to obtain conviction was to approach the middle part of each with the outer dry part of the lips. It was possible to keep the copper-covered one pressed to the lips for a second or two, but not so with the steel-covered one. The heat was so considerable in it that it was not possible to touch it for an instant without sharp pain. These temperatures were persistent.

Taking copper as having $\frac{1}{8}$ th the resistance of steel, and the rise of temperature to be in the inverse ratio of the 4th power of the diameter of the cylinders or conducting-wires (De la Rive's 'Electricity,' vol. ii. p. 224), the temperature of these two cylinders ought to be nearly the same if the force is conducted by the whole thickness instead of by the surface alone; whereas, if by the surface alone, the rise of temperature in the steel-covered one ought to be three or four times that in the copper-covered one. J. J. WATERSTON.

ON THE COLORATION OF GLASS BY SELENIUM. BY M. J. PELOUZE.

I determined, some months ago, that the yellow colour acquired by glass under the influence of carbon, phosphorus, boron, silicium, hydrogen, and aluminium was due to the constant presence of a sulphate in the glass of commerce, and that glass remains perfectly colourless under the influence of these various metalloids when it has been prepared with fluxes completely free from sulphur. Hence the coloration in question must be owing to the sulphur exclusively; and I have proved this to be the case by directly colouring pure or impure glass with sulphur or a sulphuret.

It then became a curious question to determine whether selenium, which has every possible habit and analogy with sulphur, would also directly colour glass, and what colour it would impart to it.

I had preserved a specimen of perfectly pure selenium, which the illustrious author of its discovery gave me thirty years ago. I mixed it with the ordinary glass composition prepared with the carbonate, and obtained a perfectly transparent matter, of a beautiful orange-colour inclining to red, and resembling certain varieties of topaz, essonite garnet, and hyacinth zircon (fifth orange-red $\frac{3}{10}$, ninth shade, M. Chevreul).

I varied the proportions of selenium from 1 to 3 per cent., and always obtained a colour of the same shade and intensity. Some purchased selenium gave the same result.

This experiment proves that the long-known analogies between sulphur and selenium extend to their action upon alkaline and earthy silicates, and that these two metalloids directly colour glass.—*Comptes Rendus*, October 16, 1865.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

FEBRUARY 1866.

XI. *A Sketch of the Doctrine of Colour-disease.*
By Dr. E. ROSE, *Lecturer on Surgery in Berlin**.

BEFORE I draw the attention of the reader to a simple and very convenient means of exactly ascertaining the nature of the colour-blindness in colour-blind persons, it may be convenient to give a sketch of the doctrine of colour-disease, which I have derived from investigations which are published in Virchow's *Archiv*, in v. Gräfe's *Archiv*, and in the Berlin *Klinische Wochenschrift*. I have up to now investigated thirty-three cases of artificial colour-disease produced by taking santonic acid, two cases produced by atropine, five by liver complaints, one by disease of the kidneys, one case of night-blindness, fourteen cases of congenital colour-disease (the so-called Daltonism), and three cases of congenital colour-blindness. The following are the results of the investigation.

Excluding hallucinations and phantoms of vision, which, only depending on the action of the brain, are recognized by their not being connected with the incidence of light, and hence do not occur partially, persons actually colour-diseased† come under two great divisions, which I will call colour-blind and colour-mistaken.

I call colour-blind one whose retina has lost sensibility for coloured light.

Without an exception, it has been found that with the colour-blind it is always light of the greatest or of the least refrangibility that first becomes imperceptible.

* Translated from Poggendorff's *Annalen* for September 1865.

† Colorations of the media of the eye, which, according to older assumptions, possibly produced something similar, do not occur in nature. The strongest which occur are not adequate. Virchow's *Archiv*, vol. xxx. p. 442.

It has, secondly, been found, with the same invariability, that as the disease increases, the patient ceases to perceive only that light which had previously the greatest or smallest refrangibility among his visible rays.

Colour-blindness is always characterized by a shortening of the spectrum, and never by an interruption*. A complete and accurately defined spectrum thus forms by its extent a measure of the degree of colour-blindness.

The first and most pronounced case of colour-blindness affected myself in August 1858. A brilliant spectrum, the most beautiful I have seen, was used as a measure. My late friend, Dr. Otto Hagen, used it in his beautiful investigations on the absorption of light in crystals, the results of which are partially given in vol. cvi. of Poggendorff's *Annalen*. The spectrum, as represented therein, fig. 2, plate 3, was produced by reflecting a solar ray from the silvered mirror of a heliostat, through two slits placed a metre apart, upon a flint-glass prism placed immediately behind, by which it was decomposed in a dark chamber. At a distance of eight metres its constituents were observed by the telescope of a theodolite with cross threads. By a micrometer-arrangement the prism can be turned at will, and the red or the violet end introduced. The observations were made on bright summer days, and only a transient little cloud obscured the sun. Apart from such interruptions the spectrum was seen in its entire splendour, interrupted by about thirteen of Fraunhofer's lines, terminating sharply on both sides on the black background of the telescope, and so long that in the field of view the entire red could not be seen at once. Its length was sometimes momentarily altered at the red end; and on different days its length was different, without its being always possible to detect with the naked eye the fine haze which covered the sun, and which then sometimes agglomerated into clouds. The result was tested and repeated on different days, but we always agreed in the position of the cross threads at the ends of the spectra.

The result was different when I had taken ten grains of santonic acid. Two hours afterwards my spectrum ended at the line B, while Hagen placed the cross further, where *a* must have been, in a space which was perfectly dark to me. It remained thus for some time; though once for about half an hour he observed the spectrum to end at B, while at the same time I found it to end at C. On the next day the cross was placed by both in the same position.

It follows from this that yellow vision, for only this occurred†,

* "On the Action of the essential Constituents of *Santonicum*," in Virchow's *Archiv*, 1859, vol. xvi. pp. 223-253; and vol. xviii. pp. 15-43.

† Case 12 in Virchow's *Archiv*, vol. xvi. p. 239; vol. xviii. p. 18.

corresponded to a blindness for red. The shortening of the spectrum increased during the state of narcosis, but it must be left an open question whether the narcosis or the condition of the sun was more the cause; for the spectrum had in fact become somewhat shorter, in consequence of the haze about the sun.

In order to be able to control the length of the spectrum in each case, it appeared to me better to continue the experiments with the objective spectrum. My arrangement consisted in allowing a solar ray from the mirror of a solar microscope to fall through a slit into a perfectly dark room, where it first fell on a lens with a focal length of 3 feet, and then on a flint-glass prism. The spectrum was received on a paper screen, or, better, on a second, after it had passed through a slit in the first, in order to cut off the dazzling parts, especially the yellow. While I displaced the first screen the person experimented upon always closed his eyes. The first screen was so placed that the larger Fraunhofer's lines were distinctly seen. They were used as a scale, and a place was designated by estimation as $\frac{1}{2}H$, when it was midway between G and H.

After 5 grains of santonic acid the end of the spectrum in one case (XXXIII.)* was successively

at $\frac{1}{2}H$ instead of H (as it was frequently in other cases),
subsequently at G instead of $\frac{1}{2}H$,
finally at G instead of H.

Of course, in artificial colour-blindness, I always compared my eye both before and afterwards with that of the person experimented upon.

Although the spectrum terminated so beautifully, it had the great defect that, from the fluorescence of the paper beyond the violet, there was often a foggy colourless zone with Fraunhofer's or rather Stokes's lines, which often rendered uncertain an accurate determination of the limit. Fluorescence explains why to some during narcosis the violet end did not appear shortened and black, but whitish (cases XXXIII., XXXV., XXXVI., XXXIX.), since, even within the violet and blue in the spectrum, fluorescent substances (to which paper and the skin belong) effect a lowering of the refrangibility. By means of the portion of diminished refrangibility the violet-blind sees the violet part of the spectrum like a dull zone, just as a healthy person sees the ultra-violet.

As, further, in all these experiments there was only an alteration at the blue end, in opposition to the first experiment†, this defect was peculiarly important.

* Virchow's *Archiv*, vol. xviii. p. 20.

† In case 41 also, red-blindness occurred, after santionate of soda.

It may here be remarked at once, not only that ultra-violet can be seen indirectly in the objective spectrum by a lowering of the refrangibility, but that I know a colleague who even in the subjective spectrum sees the ultra-violet, and, accordingly, actually sees it*,—a power of seeing ultra-violet which presupposes a heightened sensitiveness of the retina, as in colour-blindness it is diminished. In any case it was seen that the subjective spectrum alone could be used for investigating violet-blind persons.

While thus the results with the objective solar spectrum were not up to my expectations, the difficulties in its use were almost insuperable. If the weather was fine, no one had time or wish to take santonine; several times when I took it myself a storm followed immediately. If weather, leisure, and will were combined the narcosis failed, or only occurred when hunger drove away the person investigated. In addition to this was the trouble of setting up, which renders useless this mode of investigation, however pleasant was the brightness or desirable the natural scale which is afforded by the Fraunhofer's lines. In further experiments I subsequently found the following kind of self-measurement the most convenient.

It was established that yellow vision in the santonine intoxication depends upon a shortening of the spectrum, upon colour-blindness, and that the shortening might take place at either end. The greenish-yellow colour of all impure colours was easily explained by this want of sensitiveness to the complementary colours in the mixture. The question was to determine with still greater certainty whether the shortening was fixed or moveable.

For this purpose the most beautiful, but unequally long and variable, solar spectrum was not so well fitted as an artificial one; only it is difficult to have an artificial spectrum with sharp ends, and we lose, moreover, our natural scale.

As the source of light, a spermaceti candle was used; as spectrum, a diffraction-spectrum; as scale, the intervals between its separate bands of colour (spectra). It is known that, on looking at a source of light through a grating of 2000–3000 lines to an inch (diamond-ruled lines on glass), the light is seen surrounded at some distance on each side by a tolerably pure spectrum, followed at some distance again by a band of colour which gradually shades off. This band of colour is produced by the circumstance that the dark intervals between the individual spectra decrease in breadth on the outside; hence the violet of the third covers the red of the second spectrum to form a purple zone, and the others quite

* Compare "The Visual Delusions in Jaundice," together with an Appendix on the sense of colour in nocturnal blindness, and the action of picric acid on the eye. Virchow's *Archiv*, vol. xxx. p. 442.

disappear. By interposing a cobalt glass, which only leaves the ends of the spectra, we can be easily convinced of this. The third violet lies then between the second violet and the second red.

Since then, I have experimented upon colour-blind people by allowing them to look in the dark room at the spermaceti light through two parallel gratings (with 50 to 100 lines to a millimetre) at the distance of distinct vision, and then allowing them to describe or, still better, draw what they saw. The interposition of a slit was useless.

I found now*, and first of all in my own case†, that, while healthy persons see two dark spaces on each side of the light, persons seeing ultra-violet see only one; and colour-blind see three, four, and even six. It was easy to show this in the case of colour-diseased persons of all kinds.

A person suffering from jaundice from acute atrophy of the liver saw three‡; and this number was seen by a sick person with gastro-duodenal catarrh and jaundice, and by a person with chronic enlargement of the liver, jaundice and albumenuria. A patient with cancer of the liver and jaundice saw four§. Among the Daltonists

some saw two (Messrs. A, C, E, F, J);
some three (Messrs. B, D, H);
others four (Messrs. G, M);
others five (Messrs. K, O);
one six (Mr. L)¶.

* All these patients, of whom many were investigated at totally different times, exhibited a constant colour-blindness. It is otherwise in the santonine intoxication, as was probable from previous investigations. During narcosis the number of spaces rises and falls from two to

three (cases 46, 50, 51, 56); in other experiments to
four (cases 44, 45, 57); in some, finally, to
five (cases 43, 46), and in one case to
six (case 49)¶.

From these results it might appear as if there were colour-diseased persons who are not colour-blind. But I have pointed out in 1860 that the limitation to two intervals is no proof

* "On Colour-blindness by taking Santonic Acid," Virchow's *Archiv*, vol. xix. pp. 522-587, and vol. xx. pp. 245-291.

† Ibid. vol. xix. p. 524, case 40.

‡ Ibid. vol. xxx. p. 444.

§ Ibid. p. 443.

¶ The examination of Daltonists is found in a paper on permanent visual delusions in v. Gräfe's *Archiv für Ophthalmologie*, vol. vii. pp. 72-109 (1860).

¶ The cases are numbered throughout in the various papers in Virchow's *Archiv*. From 52 downwards they are found in vol. xxviii. pp. 30 to 82 in "Hallucinations during Santonine intoxication."

that their spectrum is not shortened, that the red end takes up so much less room than the blue, that on this account a shortening at the red end might thus perhaps escape notice.

It can only be said that when there are more than two spaces the spectrum is certainly shortened, and not conversely. That this is the case I have since then convinced myself.

It was now the question to be able to ascertain, by means of a simple instrument which could be carried in the waistcoat-pocket, the existence of a shortening of the spectrum—that is, to show colour-blindness in the case of the colour-diseased. It was also of interest to ascertain whether with two intervals a shortening of the red can occur—whether, in fact, there are colour-diseased persons without colour-blindness, and how far the spectrum in the case of those with six intervals may be shortened.

At present, when the spectrum-apparatus of Bunsen and Kirchhoff is everywhere met with, it is easy to be convinced on the subject. In order to have a sharply terminated spectrum and to be able to deaden the bright yellow, one of the large apparatus with rotating ocular telescope is necessary. All false light must moreover be carefully excluded by black cloths. A spectrum is thus obtained which even in artificial light is tolerably sharply defined, is subjective, and is hence not liable to fluorescence, and, by using simple chemically coloured flames, can always be obtained of the same length.

As a scale, the reflected image of the photographed scales can be used instead of the Fraunhofer's lines, or, better still, the natural scale as offered by the inspection of various metallic spectra. In this way the frequent imperfection of the red of the solar spectrum is avoided, which was probably formerly often the reason why defects there were overlooked. Looking at Bunsen's plate 5, nothing more convenient can be imagined for deciding the question whether there has been a shortening than the preparation of a potassium spectrum.

A preliminary investigation, for which Dr. Alexander Mitscherlich, by means of his apparatus, kindly prepared the necessary constant spectra, confirms sufficiently what I supposed and stated years ago.

After the sodium-line D had been fixed on 12 (as in Plates I. and II., vol. xxviii. of this Magazine), a potassium spectrum was produced by the vapour of cyanide of potassium heated in hydrogen; by a platinum wick, a chloride-of-strontium spectrum; by a cigar-stump, a lithium spectrum; and finally, by a simple lamp, a coal-gas spectrum*. The results were as follows:—

* Compare "Contributions to Spectrum-Analysis," by A. Mitscherlich, vol. cxvi. p. 499 [abstracted in *Phil. Mag.* vol. xxiv. p. 519]; and "On Spectra of Compounds and of Simple Bodies," by the same, vol. cxxi. p. 459 of Poggendorff's *Annalen* [translated in *Phil. Mag.* vol. xxviii. p. 169].

1. Colour-blind persons with two intervals in the grating-spectrum may in fact be blind to red, and indeed blind to potassium-red. Mr. F* was tried, a Daltonist, who in August 1860 saw two intervals.

He saw the red lithium-line (at 13·7), the blue potassium-line (at 2·5 of the photographed scale), but not the red potassium-line (at 15); even when its position on the scale was pointed out, it was impossible for him to make it out.

With the gas-spectrum a small shortening was also seen.

2. Colour-blindness may extend over half the spectrum. On one occasion Mr. B† was tried, a Daltonist, who five years ago saw three spaces. He no more saw the red potassium-line than did F‡, not even when the place was indicated; the blue very well.

He saw the chloride-of-strontium spectrum as we did (between 13 and 14), at any rate when his attention was directed to it.

The spectrum of the gas-lamp ended at 2·9 in all our cases.

Mr. M was then tested§, who varied at that time in his statements, on different days saw between four and five intervals, and in other respects showed great variation in his perception of colour, now only saw three spaces. He saw the red potassium-line, and the red chloride-of-strontium immediately, but the violet line not even when its place on the scale was indicated.

A good gas-lamp spectrum ended in his case exactly at 6; with myself, M. Mitscherlich, and Mr. B at 2·9. A second investigation some days later gave the same result.

Mr. L was lastly tested||, who then saw six intervals.

A good gas-spectrum, which, as has been stated, began at 2·9 and extended to 15·5, ended in his case on the one side at between 5 and 6, nearer 5; on the other at about 13.

In the course of the observation he made the characteristic remark that from 3 to 1 the field of view was brighter than from 3 to 5, yet by no means so bright as between 5 and 7; as might be expected, because the field of view is somewhat brightened by the flame for the photographic scale. He further stated that he could see the figures of the scale 14–18 very distinctly, but scarcely 12 and 13; while I and others could perceive only an indication of 14, because our gas-spectrum extended beyond this figure.

Of the potassium-spectrum he saw neither the red nor the blue line, not even when the position was shown (15 and 2·5). Of the chloride-of-strontium spectrum he saw only the first pale orange bands at 12·4; not one of the five red ones, which lie between 12·7 and 14.

* v. Gräfe's *Archiv*, vol. vii. p. 87.

† Ibid. p. 80.

‡ He was not present.

§ Gräfe's *Archiv*, vol. vii. p. 104.

|| Ibid. p. 99.

It follows that colour-blindness can occur with regard to both ends of the spectrum, and reduce it to one-half (about $\frac{8}{17}$)—a result which will convince even one who has only bad instruments at his disposal.

Apart from the circumstance that such people are naturally blind in light of another colour, any colour-blindness is of itself amblyopia, a weakness of vision which shows itself externally in the visage, and for the most part so pronounced that it has led my friends, healthy in other respects, to visit oculists,—a result not surprising, as almost (in Mr. M for example) half the source of light is inactive, from which secondary disturbances again result. But that is not the only evil of colour-blindness. Is it immaterial whether with progressive colour-blindness the world finally appears in almost homogeneous light? Is it an advantage to see everything monochromatic? Is the chromatic aberration of the eye a defect which is common to humanity?

As I was walking through the streets with Mr. M, whose power of accommodation and sharp-sightedness are enviable, he suddenly grasped my arm. The sun had shone out, and without being led he could go no further. After he had gradually accommodated himself to the dazzling brightness by closing the eyelid, he continued the walk with continual spasm of the eyelids and nystagmus, which he did not before have—like a rabbit which has the magnesium light reflected on its retina by a mirror. I had previously noticed that his younger brother had nystagmus; and his mother had assured me that four absent ones had it too. Hence they had consulted all the most important oculists, and, as this was without success, some less important ones also—not without pain, so that now it gave me the most terrible trouble to get hold of the brother, who intentionally kept out of the way. In fact he also saw five intervals, and was just like his brother; so that I do not hesitate to consider chronic spasm of the eyelids and nystagmus a frequently overlooked consequence of colour-blindness. In any case we must assume that man naturally sees nothing distinctly, because for each homogeneous constituent of the light he must accommodate differently, just as the telescope must be adjusted specially for each Fraunhofer's line. The chromatic aberration of the eye is the normal condition, and it is not without injury that this beneficent shield is wanting to the colour-blind. Weakness of sight and dazzling, with their host of secondary ailments, are the direct consequences of colour-blindness, quite apart from chromatic delusions, which are not so serious as in the case of colour-mistake*.

The result of this investigation might be summed up by

* *Farbenirrsinn*, implying that the mistake is one of sensation, not of judgment.—ED.

saying that

Messrs. B and F are blind to potassium-red,

Mr. M blind to potassium-blue,

Mr. L blind to strontium- and potassium-red,—

a designation which, in spite of its length, has the advantage of being absolute and independent of the instrument.

All these gentlemen I investigated five years ago, and have described them in v. Gräfe's *Archiv*. They are selected because they represent all classes: for Messrs. B and F are plane Daltonists, and therefore colour-mistaken; Mr. L a linear Daltonist, and therefore totally colour-mistaken; Mr. M not at all colour-mistaken, but only colour-blind.

For the sake of intelligibility, some communications on sensational colour-mistake, that second great division of colour-diseases, may be permitted to me.

The first kind of colour-mistake which I investigated, and first in my own case, concerned what I have called "violet vision"* in the advanced santonine-narcosis, in which all objects, the darker they are or are illuminated, the more violet do they appear. It is often combined with violet-blindness; and yellow vision with violet vision. If this is so, the following striking experiment† can often be made:—"One who does not see the blue end of the spectrum thinks a homogeneous yellow common-salt flame seen through a homogeneous yellow glass to be yellow; seen through three or four such glasses, to be violet."

Hence there is a complete inversion of the three fundamental constituents of the perception of a colour—perception of purity, tone, and strength. This experiment shows that such a person confounds two opposite complementary colours of unequal intensity, and considers two unequal intensities of one tone of colour to be opposite colours. This is only one example of the numerous confusions of colour which occur in the santonine intoxication in the case of violet vision, but it is the characteristic one. If a couple of confused colours have been determined by measurement, the others may be calculated beforehand, to which I will here only advert‡.

Formerly I called those who thus systematically confound colours colour-confounders; I would rather now call them colour-mistaken, not merely for shortness' sake, but also because healthy people, according to the degree of illumination, confound colours, though but few; colour-blind people still more; and the characteristic of the colour-mistaken as compared with them consists in a further mistake in the designation of the colour.

* Virchow, vol. xix. p. 532.

† Virchow, vol. xx. p. 278. A strontium-spectrum (case 45) appeared to consist of purple, violet, and green; when looked at through a dark blue glass, only of green.

‡ Virchow, vol. xx. p. 245.

The chaos of contradictions in the older and even in the newest statements as to defects in the sense of colour, colour-blindness, and Daltonism, is partly explicable by the mode of investigation, and partly by the circumstance that the two chief diseases have not been discriminated.

Both may occur together, as in the highest degree of the santonine stimulus, in acute yellow atrophy of the liver, nocturnal blindness, congenital colour-disease. All Daltonists are colour-mistaken; from the result of the above-mentioned spectrum investigation they are probably all colour-blind; hitherto, it is true, it has not been shown in all cases.

As Mr. M (and others) shows, congenital colour-blindness occurs without colour-mistake, just as the santonine-stimulation often leaves yellow-sightedness, and does not attain a violet vision. Just so among five cases of colour-diseases after liver complaints, there was only one, which was quickly fatal, in which colour-mistake supervened, and that only momentarily. The other cases went no further than colour-blindness (two cases of catarrhal jaundice, one of hypertrophy of the liver, one of cancer of the liver). It follows thence that sensational colour-mistake is the more serious complaint. Further experiments must show whether, in the case of jaundice, by diagnosis of the kind of colour-disease, the gravity of the case can be previously determined, as almost seems to be the case.

Colour-mistake may in general be divided into simple and complete. One who sees violet in the santonine stimulation is simply colour-diseased; of the great number of colours he confounds, there is only one complementary pair of colours. This is the case with those Daltonists whom I have called *plane**, because their congenital colour-system may be represented by a plane, just as that of a healthy man by a cone. Different from these are the Daltonists whom I have called "linear," as, for instance, Messrs. K†, L‡, and O, and probably also the patient with disease of the kidneys whose examination remained incomplete,—linear, because their congenital mass of colours can be represented by a line.

It is characteristic of these completely colour-mistaken persons, that to them all pairs of complementary colours, of a determinate different intensity, always seem equal; to a singly colour-blind only one pair.

All colour-mistaken persons have this in common, that no two have hitherto considered a pair of colours agreeing in intensity and tone to be equal.

* Gräfe's *Archiv*, vol. vii. p. 92.

† Ibid. p. 98.

‡ Ibid. p. 99.

It has further been established as a law, that each colour-blind person, in a complementary pair of colours that he sees equal in this his characteristic colour-equation, has always a feebler sensitiveness for the blue or red side than for the green and yellow. Experience seems, finally, to show that all colour-mistaken persons are red-seers, with the exception of violet-seers under the santonine stimulation.

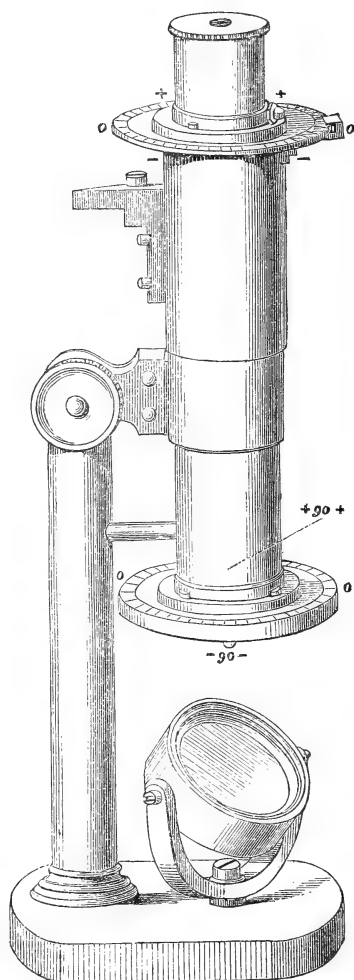
For investigating colour-mistaken persons I use my colour-measurer*, as now constructed by M. Oertling according to my direction, for practical use even at the bedside. In a polarizing-apparatus (two Nicols with divided circles) there are fixed an achromatic doubly-refracting spar prism and a rock-crystal plate cut perpendicularly half a centimetre thick. The image of the diaphragm is seen through it always double, the two near each other and of complementary colours. The colour depends on the position of the upper Nicol (O); the difference in the intensity of the two images on that of the lower one (U).

The colour-stamp of plane Daltonists gives, in the form of an equation, the readings on both divided circles, at which both images appear to them quite identical.

Such colour-stamps are, for instance, for

Messrs.

A.	O + 22 = U	- 57
B.	O + 12 = U	53
F.	O + 15 = U	60
G.	O + 13 = U	60
H.	O + 12 = U	50
J.	O + 17 = U	48
N.	O + 16 = U	52



* Compare woodcut. I am quite ready to have the small correction of the instrument determined by a Daltonist of my acquaintance, so that the instruments may be comparable.

I scarcely need repeat that I have hitherto in vain confronted some of these gentlemen—this time, for instance, F, G, J; in no case has the equation been the same for two patients.

While a healthy or a colour-blind person seeks in vain for such a colour-equation, a colour-mistaken person is ready in a minute. A completely colour-mistaken person differs from the ordinary singly colour-mistaken in the circumstance that the latter finds an equality in only one position. For the former, the upper Nicol may be placed as we like; by turning the bottom one he effects an equation. Yet the reverse is not the case: the lower arm cannot be turned beyond a certain difference of brightness.

Mr. L, for instance*, gave the following equations:—

1. O-10 = U-56½	19. O 10+ = U-61
2. O-20 = U-61	20. O 20+ = U-62½
3. O-30 = U-63½	21. O 30+ = U-64
4. O-40 = U-66	22. O 40+ = U-58½
5. O-50 = U-58½	23. O 50+ = U-53½
6. O-60 = U-52½	24. O 60+ = U-49
7. O-70 = U-46½	25. O 70+ = U-40
8. O-80 = U-36½	26. O 80+ = U-36
9. O-90 = U-32½	27. O+90+ = U-30½
10. O 80- = U-27	28. O+80 = U-25½
11. O 70- = U-24	29. O+70 = U-21½
12. O 60- = U-20½	30. O+60 = U-19½
13. O 50- = U-22½	31. O+50 = U-20
14. O 40- = U-29½	32. O+40 = U-24
15. O 30- = U-36½	33. O+30 = U-30½
16. O 20- = U-45	34. O+20 = U-38½
17. O 10- = U-50	35. O+10 = U-45½
18. O 0± = U-57	36. O± 0 = U-54½

And Mr. O,

37. O+30 = U+34	43. O 30- = U+28
38. O- 0 = U+57	44. O 0± = U+56½; 58½
39. O-30 = U+57	45. O 30+ = U+67
40. O-60 = U+45	46. O 60+ = U+52
41. O-90 = U+31	47. O 90+ = U+34
42. O 60- = U+20	48. O+60 = U+22

* The colour-measurer usually stands on the window-sill in such a manner that the support is on the right hand, so as to be read off on the left. The quadrant turned towards the observer I call the positive. If the mark is on the right side of the observer, I place the number of degrees of the reading on the right of (behind) the cipher,—for instance, O 24—.

Mr. K,

49.	O	$0 \pm = U$	42 +
50.	O	$30 - = U$	68 +
51.	O	$60 - = U$	54 +
52.	O	$90 - = U$	43 +

It is thus seen that totally colour-mistaken people are not so exact in adjusting as the singly colour-mistaken,—possibly from a little hastiness if they are tired by the investigation, but partly from amblyopia and atrophy* of the visual nerve, which probably always accompanies so high a degree of colour-disease. This is also seen in the circumstance that the position of the lower Nicol varies within certain limits.

These cases can thus be characterized by a group of equations which indicates the results for both limits, and a mean value of the lower Nicol; thus, for instance, for

$$\text{Mr. L, } \left\{ \begin{array}{l} U - 30 = O + 89 -; + 32 - \\ U - 45 = O + 16 -; - 72 + \\ U - 60 = O - 6 +; - 44 + \end{array} \right\}$$

$$\text{Mr. O, } \left\{ \begin{array}{l} U + 30 = O \pm 90 \pm; + 41 - \\ U + 45 = O + 20 -; - 73 + \\ U + 60 = O - 9 +; - 53 + \end{array} \right\}$$

Remembering that Daltonists may be at the same time colour-blind—indeed blind to red and to violet, and both to different degrees—it is easy to conceive that no Daltonist sees colour exactly like another, and what different perceptions of colour the same excitation must produce in different individuals.

Among fifty-nine colour-diseased I have not found any two who exactly exhibit the same sense of colour.

It seems almost a matter of course, and it follows, indeed, from other experiments and reasoning, as I showed some years ago†, that Young's theory of colour, whether modified or not, is irreconcilable with this; for it only allows three kinds of colour-blindness, or, taking in the combinations, six.

* This is the reason why I am not certain whether Mr. K is affected in the same way as Messrs. L and O; for each time I have had to abstain from further investigation.

† Gräfe's *Archiv*, 1860, p. 89; and Virchow's *Archiv*, vol. xx. p. 282.

XII. *On the Bands formed by the Superposition of Paragenic Spectra produced by the Grooved Surfaces of Glass and Steel.*—Part II.
By Sir DAVID BREWSTER, K.H., F.R.S.L. & E.*

[With Two Plates.]

IN the preceding paper, I have described the bands produced by gratings or grooved surfaces with 500 divisions in an inch when the two grooved surfaces are in contact, and the grooves in the one slightly inclined to those in the other.

The following results were obtained with two gratings, one of which had 2000 and the other 1000 divisions in an inch.

1. When the surfaces are in perfect contact and the grooves parallel, the bands seen on the united surfaces, either with a lens or by ordinary vision, are very irregular and are parallel to the grooves. They are seen only on the 2nd, 4th, 6th, &c. spectra on each side of the luminous bar or disk.

By turning the nearest grating slightly to the right from the azimuth 0° , the bands fall back to the left, increasing in number, and descending with their concave sides downwards into distinct serrated black and white bands nearly perpendicular to the grooves. When the nearest grating is turned to the left, the bands descend towards the right, with their concave sides upwards, till they become nearly perpendicular to the grooves. In all these positions the bands are twice as numerous on the fourth spectrum as on the second, and thrice as numerous on the sixth as on the second; and when the grooved surfaces are perfectly parallel, the bands are immovable on the grooved surfaces at all angles of incidence.

2. When the grooved surfaces are separated by the thickness of one or both of the plates of glass, the bands are very indistinctly seen, and they seem to diminish in size with the distance of the grooved surfaces; but this is not certain, owing to the difficulty of fixing the plates with the grooves at the same inclination to each other.

Similar bands were seen on the united surfaces of gratings of 2000 and 2000, 1000 and 1000, 500 and 500, 1000 and 500, and 2000 and 500 divisions in an inch, but always less distinctly when the grooved surfaces are separated by the thickness of one or both of the plates.

The beauty and distinctness of these bands depend upon the skill with which the gratings are ruled. In several of the gratings which I possess, the phenomena I have described can hardly be recognized.

When the combined gratings have the same number of divi-

* From the Transactions of the Royal Society of Edinburgh, vol. xxiv. part 1. Communicated by the Author.

sions, such as 1000 and 1000, the bands are seen upon all the spectra, and sometimes very faintly on the luminous disk; but when one of the gratings has twice the same number of divisions as the other, such as 2000 and 1000, the bands appear, as already mentioned, only on the 2nd, 4th, 6th, &c. spectra. In such combinations the 1st, 3rd, 5th, 7th, &c. spectra of the 1000 grating have no corresponding spectra in the 2000 grating with which they can interfere, whereas, when the divisions in both are the same, all the spectra of the one are superposed upon all the spectra of the other, and therefore bands are produced upon each of them.

In like manner, if the number of divisions in the one grating is to those on the other as n to 1, n being a whole number, the bands will appear only on the spectra $n, 2n, 3n, 4n$, &c.

When a grating of 1000 is placed above one of 2000, I have observed faint bands upon the spectra 1, 3, 5, &c., of the 1000 grating, though none of the spectra of the 2000 grating could interfere with them. These bands are more numerous than those between which they lie, and are doubtless produced by the interference of spectra reflected from the plane surfaces of the glass plates with those seen by transmitted light.

When the gratings of 1000 and 2000 are placed at a small angle, as in Plate II. fig. 1, the grooves being parallel to AM , and the light incident perpendicularly, the bands on the left-hand spectra are parallel and rectilineal, and highly *purple* and *green*, as in fig. 2.

By turning the gratings round AM as an axis, in the direction from D to B , the bands descend from m , as in fig. 3, till they become parallel vertical lines, increasing in number and less coloured, as in fig. 4, the number of bands on the second left-hand spectrum being double those on the first.

When the rotation is in the opposite direction, from B to D (fig. 1), the bands rise from n (fig. 5) till they become parallel and vertical as before.

The opposite effects take place when the gratings are placed as in fig. 6, AM and CS being coincident, and when we observe the spectra on the right hand of the luminous disk. The bands now descend and ascend from the same points m, n , now on the outer side of the spectra.

When the two edges AM, CS of the gratings are not parallel, as they are in fig. 1, but inclined at a small angle, $AMSC$ (fig. 6), then if, when the fringes are parallel at a perpendicular incidence, we turn the gratings round AM as an axis from B to D , the fringes descend from m , becoming smaller and smaller till they are parallel and vertical; but when the gratings revolve from D to B , the fringes become larger and larger, less nume-

rous, and more coloured, till they are finally parallel to A M, the fringes being twice as numerous on the second spectrum as on the first.

When the grooves are perpendicular to A M, as in fig. 7, the bands are faint and indistinct. The light being incident perpendicularly and the gratings turned round A M on a plane perpendicular to A M, the fringes do not increase in number or greatly change, if the motion is accurately in a plane perpendicular to A M.

When the gratings are turned in the plane of the horizon passing through A M, the side N M approaching the eye, the fringes on the left-hand spectra descend, increasing rapidly in number; and when the side M N recedes from the eye, the fringes ascend, increasing in magnitude and diminishing in number, and are highly coloured. At a certain angle they become parallel to the grooves, when by continuing the rotation they move downwards, increasing in number, and again becoming parallel to the grooves.

In the preceding experiments the bands are seen on the surface of the gratings; but when the grooved surfaces are in contact and the grooves parallel, bands of an entirely different kind are seen, not on the surface of the gratings, but by rays diverging from the luminous disk. If we use a long and narrow bar of light, such as the opening between the window-shutters, then, when the grooves are parallel to the bar, and the grooved surfaces perpendicular to the plane of incidence, the bands are parallel to the bar and its spectra. By inclining the grooves to the luminous bar, the bands are inclined to the spectra, dividing each of them into a great number of spectra; and at an azimuth of 45° the bands become perpendicular to the spectra. At all these inclinations the bands on the second spectrum are double those on the first, the number increasing in arithmetical progression on succeeding spectra.

When the angle of incidence is increased, the bands increase in number, but very slightly with gratings of 1250 divisions in an inch.

By increasing the distance between the gratings, the bands also increase in number.

Bands similar to those now described are produced with interesting phenomena by a single grating placed (as in fig. 8) so that the image of the grooved surface A B, reflected from M N the lower surface of the glass, is superposed as it were upon the grooved surface itself.

1. When the plane of reflexion is perpendicular to the grooved surface, and the grooves in the same plane, the bands on the spectra are parallel to the bar of light A B, those on the second

spectrum being double those on the first. They are seen at all angles of incidence, and are larger and more distinct at small angles.

When the grating is turned round in its own plane at any angle of incidence, so that the grooves form different angles with the bar of light, the bands cross the spectra and become perpendicular to them in the azimuth of 45° . The paragenic spectra are thus divided into a great number of spectra, the number increasing as formerly on each succeeding spectrum.

2. When the grooves are parallel to the bar of light, and the plane of reflexion perpendicular to the grooves, the bands are apparently segments of concentric circles at great angles of incidence, the radius of which increases as the angle of incidence diminishes, so that they become straight lines at a perpendicular incidence. The bands are smaller at their upper and lower ends, and those on the second spectrum are, as before, double those on the first, as shown in fig. 9.

In the spectra on the left hand of the bar of light, the concave side of the circular bands is towards the bar; and in the spectra on the right hand of the bar of light the convex side of the circular bands is towards the bar. The bands on the right-hand spectra are smaller and more numerous than those on the left-hand spectra; and yet, by increasing the angle of incidence, the bands on all the spectra increase in size and diminish in number.

If at any particular incidence we turn the grating in its own plane, the bands cross the spectra at angles increasing with the degree of rotation, and becoming smaller and more numerous. When the end of the grating nearest the eye (A, fig. 8) ascends, the fringes, great and small, diminish and become more distinct, and the centres of the circles descend. When the grating is turned in the opposite direction, the centres of the circles ascend.

In the principal gratings which I possess, when upon thin glass*, including those of 1000 and 2000 in an inch, these circular bands are accompanied by another system of circular bands, convex to the luminous bar when seen on the left-hand spectra, and concave to it when seen on the right-hand spectra; but what is remarkable, they are smaller and more numerous on the first spectrum than on the second, as shown in fig. 10. They

* These bands are not seen on a beautiful Munich grating, kindly lent me by Professor Stokes, having 3750 divisions in an inch. As the bands become smaller with the thickness of the glass, their absence in this grating arises doubtless from its great thickness, which is 0.158 of an inch, the thickness of the gratings upon which they appear being about 0.04.

are best seen when the principal circular bands cross the spectra obliquely.

In the preceding experiments with one grating the grooves of the reflected image are necessarily parallel to those of the real grating, owing to the parallelism of the surfaces of the plate of glass, and therefore they cannot exhibit the result of superposing two systems of grooves inclined to each other. This condition, however, may be obtained by drawing the grooves on the faces of a prism with a small angle, or by placing a fluid prism between an ordinary grating and a plate of thin parallel glass, which would enable us to vary the inclination of the two sets of grooves. A better arrangement, however, is to place the grating AB (fig. 11) upon a polished metallic surface, MN. A ray from the luminous bar at R, incident on AC at r , reaches the eye at E after reflexion from the steel surface MN, so that the reflected image of the grating AB is superposed as it were on the direct image.

When the grating AB, of 1000 grooves in an inch, is laid upon a steel surface MN, and the grooves are in the plane of incidence, the paragenic spectra of a luminous bar are covered with bands, not serrated, parallel to the spectra, exhibiting all the phenomena already described as seen by reflexion from a single grating.

The bands are of the same size as with a single grating when the grooved surface is uppermost, but they are very much larger when the grooved surface is in contact with the steel.

When the grooved surface is slightly inclined to the steel surface, as in fig. 11, and the grooves parallel to the plane of reflexion, a double system of hyperbolic bands is seen, as in Plate III. fig. 12, having one asymptote coincident with the bar of light and the other at right angles to it. One of the systems of hyperbolas is on one side of the bar and the other system on the other side, the number of bands on the second spectrum being double those on the first.

When the grooves are inclined to the plane of reflexion, by turning them to the left or to the right, the double system of hyperbolas moves to the left or to the right, the curves of each system crossing the spectra, as in fig. 13, and being, as before, twice as numerous on the second as on the first spectrum on both sides of the bar. By increasing the inclination of the grooves to the plane of incidence, the system of hyperbolas moves further to the left or to the right.

When the bar of light is placed at E and the eye at R (fig. 11), the system of hyperbolas is inverted, as in fig. 14.

It is curious to observe the passage of the parallel rectilineal bands into hyperbolas, when the inclination of the grooved to

the steel surface commences: the parallel bands open at their lower end, as in fig. 13, or at their upper end, as in fig. 14, and change into hyperbolas. When the light was strong, I observed a second but fainter system of hyperbolas lying between the principal system and the luminous bar, and caused probably by reflexion from the second surface of the grating. The effect produced by the crossing of the bands arising from these two systems of hyperbolas was remarkable, and similar to what I had observed in combining two gratings of 500 divisions in an inch. This second system of hyperbolas was most distinct when the plane of reflexion from the surface of the steel was coincident with the plane of reflexion from the glass; and the double system was seen with grooved surfaces of 500, 1000, and 2000 divisions in an inch.

In using accidentally a steel surface that was not perfectly flat, I was surprised to observe that the bands were not hyperbolas, but circular rings varying in form and size with the angle which the grooves formed with the plane of reflexion. In order to examine this new and beautiful phenomenon, I placed the grooved surface of the grating *AB* upon a convex surface of steel, *MN*, as in fig. 15, so that the rays from the luminous body might reach the eye at *E*, after reflexion from the convex surface *MN*. The reflected image of the grating is thus superposed upon the direct image, and two systems of concentric rings are seen upon the surface of the grating. At the point of contact, *C*, and around it, are seen the rings of thin plates described by Newton, and increasing in size with the radius of the surface *MN*. Around and concentric with these, as shown at *ab*, fig. 16, is seen a beautiful system of serrated rings formed upon the paragenic spectra, the number of rings upon the second spectrum being double those on the first, as before, and becoming narrower and closer as they recede from the centre. When the first and second spectra are close to one another, the rings upon entering the second spectrum are doubled, as shown at *mmm*, fig. 17. These rings are seen only when the grooves are inclined to the plane of reflexion. By increasing the inclination they become smaller and more distinct, their size being a minimum and their distinctness a maximum when the azimuth of the grooves is 90° . When the azimuth is 0° , or when the grooves are turned into the plane of reflexion, the rings open, as at fig. 18; and when turned into azimuth 1° or 2° , those on the side *ab*, fig. 18, go back to the left, and those on the side *cd* bend into a ring, as shown in fig. 19. When the rings are again formed, they increase as the angle of incidence diminishes.

When the rings are increasing or diminishing, or passing from one spectrum to another, their centres are sometimes white, and

at other times so black as to eclipse the rings of Newton. Their colour is very variable, sometimes black, with colourless intervals, and sometimes richly coloured with the tints of the spectra on which they are seen. When the grating is pressed upon the convex surface, or raised slightly from it, the rings exhibit the same phenomena as those of thin plates.

When the ray $R R'$ (fig. 19) from the bar of light reaches the eye at E , the grooves being slightly inclined to the plane of reflexion, the hyperbolic bands are seen as in fig. 12; and when the ray $r r'$ reaches the eye at e , the hyperbolic bands are seen as shown in fig. 13; and when the eye receives all the rays between R' and r' , the direct and inverted systems of hyperbolas are seen, as in fig. 20. If, when these are seen, we look at the surface of the grating, we shall see the system of concentric rings produced by the union of the two systems of hyperbolas.

XIII. *Some Remarks on an observation of Mr. Glaisher's.* By J. M. WILSON, M.A., Fellow of St. John's College, Cambridge, Mathematical and Natural Science Master in Rugby School.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AT the Meeting of the British Association in Birmingham last September, Mr. Glaisher read in Section A a report of his observations made in balloons during the previous year. One of these was new, I believe, to those who were present, and attracted some attention—Principal Forbes, Mr. De la Rue, and others taking part in a brief discussion about it. No account of this has yet appeared, as far as I know; and as it may interest the readers of your Magazine, I shall offer you a brief statement of it, and some remarks concerning it.

That there is always a difference of reading of two thermometers, one with blackened bulb exposed to direct sunshine, and the other with bulb concealed, is well known. The latter marks the temperature of the air surrounding it; the former marks the sum of this and of the direct radiated heat which it receives from the sun. Mr. Glaisher has shown that the difference of reading of the two thermometers, when taken to great heights, instead of increasing or remaining constant, as would seem at first sight to be expected, actually diminishes, and appears as if it would diminish without limit. If a diagram were constructed in which the abscissæ were temperatures and the ordinates heights above the sea-level, the readings of the two thermometers would be indicated by two lines,—that of the former by an inclined line nearly straight, indicating a gradual decrease of temperature on

ascending; and that of the latter by a curve intersecting the line of abscissæ at some distance from the former line, and perpetually approximating to it, as an hyperbola to its asymptote, as the height increases.

Such is the fact now ascertained; and of the correctness of the observations, which were numerous, there can be no doubt. In other words, what is called the direct radiant heat of the sun diminishes, and apparently without limit, by ascending to great heights; and if the experiment could be made, the shaded and exposed thermometers outside the earth's atmosphere would show the same very low reading.

Some actinometrical observations were made which, as far as they went, confirmed the conclusion above stated. More of them, however, troublesome as they must be in a balloon, are much needed.

Accepting, then, the fact, we may indicate a few of the inferences which may be drawn from it. The very small power of the sun to melt snow and ice at high altitudes among mountains is more satisfactorily accounted for than it has previously been. All those views of the state of the surface of the moon which represent it as alternately exposed to fiery heat and intense cold will disappear. The temperature on the dark and bright halves of the half moon is the same, if there is no atmosphere. We learn one more marvellous function of our atmosphere before unsuspected. We knew that it and the aqueous vapour it bears diffused through it save us from loss of heat. We did not know that but for it, or some part of it, we should have no heat to lose. Hence, too, we learn that conclusions as to the temperatures of other planets are still more open to uncertainty, as being in this way also influenced by their atmospheres.

Two theories respecting this fact suggest themselves. First, that heat-waves do speed through space and our atmosphere, and both strike on the surface of solid bodies exposed to them, and communicate a motion to adjacent molecules not so exposed directly, which in their turn pass on the motion to others. The latter part of this motion is known as the temperature of the shaded bulb, the former as that of the exposed bulb. But the difference between the two readings indicates only the excess of the motion received over that which it sends back; and it is conceivable that the density of the surrounding gas may greatly affect the amount so sent back, so that the excess may diminish with the density of the gas, and therefore with the altitude. The density of the surrounding gas may also greatly affect the absorbing powers of the surface; and the blackened bulb, which

is an excellent absorbent with a barometer at 30 inches, may be by no means the best with a barometer at 12 inches. The second theory is equally obvious, and was suggested, if I remember right, by Mr. De la Rue, viz. that the motion we call radiant heat cannot be communicated to solid bodies except through the intervention of a gas; and that the amount so communicable depends on the density of the gas. This is a very important conclusion, and needs a fuller statement. It asserts that a form of motion is propagated by the sun, travels through the interplanetary spaces, and strikes on our atmosphere. That there the motion is of such a kind, either as regards its matter or its movement, as not to be communicable to the molecules of a solid body; but that this motion is communicated to the atmosphere, there of extreme tenuity, and by it might be transmitted to a thermometer. At greater depths in the atmosphere more and more of the original form of motion is being converted into heat, and the directly propagated motion more and more exceeds the diffused motion.

This suggests various questions of great importance in the theory of heat. For example; Is the original form of motion here spoken of that which we know as light? Is there a gradual conversion of light into heat? A comparison between the lights and heats of the sun and moon will probably indicate a negative answer to this question. But the law of conservation of force requires that there should be some motion so converted into heat. Can we learn to recognize it in any other form?

Experimental investigations may solve some of the problems, and clear away some of the uncertainties in this new branch of the subject. Is heat of all qualities capable of propagation, and of being received by absorbent surfaces *in vacuo* or in a very rare gas? Do the absorptive powers depend on the pressure? It would not seem to be difficult to answer these questions by experiments. And by observing the effect of different simple and compound gases on radiant heat, a new insight into their relations to the motions of heat and light may be obtained.

Certainly this observation of Mr. Glaisher's leads to a promising field for inquiry.

I remain, Gentlemen,

Your obedient Servant.

JAMES M. WILSON.

XIV. *On the Explosive Distance of the direct induced Current between Electrodes of the same kind.* By M. ELIE WARTMANN*.

THE greatest striking-distance which can be realized with a given electrical machine takes place when the largest possible quantity of electricity is accumulated at the point of the conductor from which the spark emanates, and when the accumulation takes place with sufficient slowness to prevent any of the fluid escaping before the discharge. As the thickness of the electric layer is modified by the dimensions and the form of the body which is brought near, it is evident that the maximum striking-distance corresponds to a given discharger. Thus Pfaff used a ball 8 inches in diameter to draw sparks 18 inches long from a machine the conductor of which was terminated by a 4-inch ball†. Faraday found that the spark extended furthest between two balls of unequal size when the smaller ball was charged positively by induction‡. M. Riess has shown that these results are not general, and depend upon the construction of the machine employed§.

In Ruhmkorff's apparatus, when the ends of the induced circuit are so far separated that the interposed air only allows the direct current generated by the opening of the battery to pass, facts similar to the above are established. It is well known, for instance, that the striking-distance between a point and a plate is greater when the point is positive than in the opposite case. But what happens when the electrodes are of the same kind and of exactly similar dimensions? I do not believe that this question has hitherto been examined.

To supply the answer, I used a bobbin, 0·352 metre in length and 0·165 metre in diameter. The power of the effects which it is capable of producing shows the perfect isolation of the layers of covered wire, which measures about 9000 metres. It is a partitioned machine, furnished with a Foucault's amalgamated platinum break, and was worked with a battery of from 2 to 10 Bunsen's elements, the large size.

The pairs of similar electrodes between which the spark was to pass were screwed to the arms of a Henley's universal discharger. Every possible care was taken that their form and dimensions should be exactly alike. These electrodes consisted successively of—

* Translated by Dr. Griffith, from the *Bibliothèque Universelle*, 1865, p. 236.

† Gehler's *Neues Wörterbuch*, vol. iii. p. 464.

‡ Experimental Researches, Series XIII. §§ 1485 to 1489. Phil. Trans. for 1838, p. 126.

§ *Die Lehre von der Reibungselektricität*, vol. i. p. 276; and vol. ii. p. 129.

1. Two straight tapering brass cones, the sides being 0·01 metre in length, and the base 0·008 metre in diameter.

2. Two brass cones truncated at the summit, the base measuring 19·18 millims. in diameter, and the truncation 2·82 millims.; the apotheme, which is 16·35 millims. in length, is terminated by a cylindrical embase 5·08 millims. in height.

3. Two brass spheres 14 millims. in diameter.

4. Two gold spheres 18·5 millims. in diameter.

5. Lastly, two brass spheres 15 millims. in diameter, fixed to the arms of a second universal discharger, which was sometimes introduced into the circuit.

When the Ruhmkorff's coil is at work, and one of the electrodes is slowly separated from the other, a distance is reached at which the spark ceases to pass. If the current be then reversed by means of the commutator, the spark either passes again, or the interval requires to be diminished before this occurs. Sometimes the difference is not well marked at first, and requires a certain duration of the electric current before it becomes manifest. When once produced, it remains tolerably constant as long as the current of the battery retains its intensity.

The apparatus may also be regulated by using the second discharger. The electrodes which terminate the first are removed to somewhat too small a distance, and the difference compensated by opening the arms of the other excitor. When the interval between the electrodes is too great for the passage of the spark, it passes or ceases between the spheres of the secondary excitor, according to the direction of the current.

Of the terminal conductors, the small pointed ones appear least favourable for experiment. The change occasioned by the direction of the induced current may be limited to a variation in the abundance of sparks. I have nevertheless succeeded in determining their cessation at distances comprised between 0·033 metre and 0·052 metre.

The proof is easier with the truncate cones and the spheres. When the electrodes consist of well-polished brass, the passage of sparks with high tension soon tarnishes them, from the formation of black spots of oxide. The difference in their conductivity which might result from this is not the cause of the phenomenon; for it remains after the electrodes have been cleaned without altering their distance. Moreover on using the gold spheres, the surface of which is not sensibly altered by the discharges, the influence of the direction of the current upon the striking-distance is shown in a very clear manner.

Neither does this influence depend upon a difference in the form of the electrodes which terminate the induced wire. This may be shown by placing in the circuit a commutator which

inverts or reestablishes the connexion of each end of the covered wire with a given electrode. The similarity of these electrodes is such that no effect is produced. A more simple method is to change directly the terminal electrodes without varying their distance. The success of this experiment requires the employment of similar and well-isolated conductors for connecting the ends of the covered wire with the arms of the discharger. The outer circuit should be free from any roughness by which the charge might be dissipated in the air before traversing the interval between the electrodes.

Thus, in air, between exactly similar electrodes kept at a constant distance, the spark passes abundantly or ceases to pass, according as one of them is or is not positive. *The spark passes when this electrode is connected with the outer positive end of the induced circuit**. I have verified the fact with a bobbin of larger size, furnished with an independent Foucault's break. If the direction of the voltaic current is interrupted very suddenly, a small spark accompanied by a peculiar sound still passes, but is not usually followed by another. At the same time the break changes the course and gives rise to a sound characteristic of the rupture of the induced current.

This interruption is not destroyed when hot and moist air is blown into the interval to be passed. If a moderate conductor, such as a wooden rod, be brought near the negative electrode, the electric distribution of the flashing spark is modified†. But this modification only lasts as long as the conductor is present.

It has been long since noticed that in non-partitioned machines the electricity does not accumulate in the same manner at the two ends of the induced wire. The induced wire and the central iron core condense the fluid upon the inner end, which does not give off a spark upon a neighbouring non-isolated conductor, as is the case with the outer end. But this peculiarity is independent of the fact that this end is positive or negative; and it is considerably weakened in the partitioned machines, simply by their construction; it appears nevertheless that the tension remains slightly preponderating in the outer knob. When it is positive, as the negative knob becomes the seat of an elevation of temperature, this twofold circumstance determines the discharge, according to a mechanism analogous to that of Sullin's card-perforator, of which M. Riess has given the explanation‡. A first partial discharge sets in motion the

* By outer end in a partitioned machine, I mean that which corresponds to the outer termination of the induced circuit of a non-partitioned bobbin when the spark is exchanged between a point and a plate.

† This property has already been studied by M. Riess. See Poggendorff's *Annalen*, vol. xcix. p. 637 (1856).

‡ *Op. cit.* vol. ii. p. 213.

interposed air and renders the vapour of the water it contains negative by friction. The path is thus open to the electric current, the access of which to the negative knob is facilitated by the expansion of the layer of surrounding air. Hence Ruhmkorff's machine differs from the electric battery, in which the striking-distance does not vary, either according to the circumstance that one of the armatures is positive or negative, or according to the nature of the connecting arc*.

On using as the discharger two wires of the same size and shape, M. de Moncel had remarked a preponderance of the positive rheophorus of the induced current, by the interposition upon this rheophorus of a very powerful resistance, as that of the secondary circuit of a Ruhmkorff's machine†. The various experiments of which I have given a summary in this memoir prove that the form of the ends of the discharger may be very different, and that the addition of a resistance towards one of the ends of the induced wire is not requisite to produce a disparity in the manner in which the discharge traverses a constant interval in the free air.

XV. *On the Construction of a Spectroscope with a number of Prisms, by which the angle of minimum deviation for any ray may be accurately measured and its position in the solar spectrum determined.* By JOSIAH P. COOKE, Jun.‡

IN an extract from a letter of the author published in Silliman's Journal, vol. xxxvi. p. 266, a method of adjusting the prisms of a compound spectroscope was described, by which the adjustment for any portion of the spectrum could be obtained with great rapidity and accuracy. A further study of the subject has shown that the method of adjustment then only briefly described admits of the highest precision, and may be applied to the exact measurement of the angle of minimum deviation of the spectrum-rays. It has been thus possible to apply the great dispersive power of the large Cambridge spectroscope in determining the relative position of the various spectrum-lines, and to secure all the accuracy of which angular measurements are capable. The value of such measurements is obvious, and, with the hope that this method will prove to be an assistance to investigators, we propose to give in this paper a description of our instrument and of the manner of using it.

* Riess, *op. cit.* vol. ii. pp. 80 & 130.

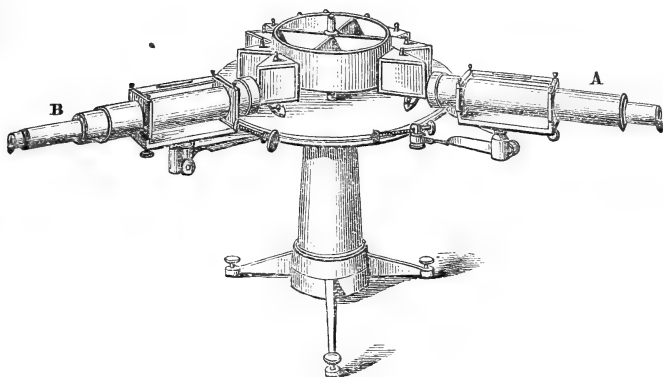
† Notice sur l'appareil de Ruhmkorff, 4th edit. p. 248 (1859). *Recherches sur la non-homogénéité de l'étincelle d'induction*, p. 89 (1860).

‡ From Silliman's American Journal for November 1865.

The general construction of the instrument is shown in fig. 1.

The two telescopes are constructed in the usual way. The

Fig. 1.

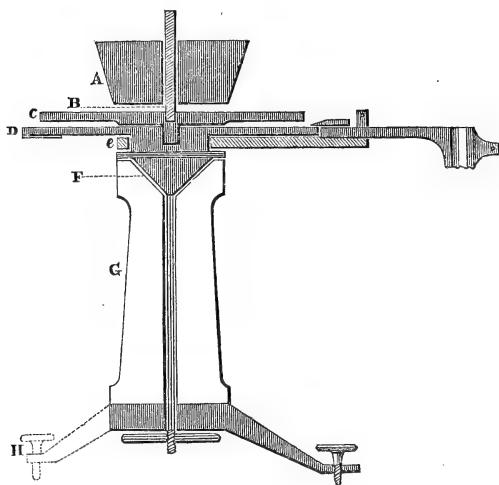


telescope A, which we shall call the collimator, has an adjustable slit placed exactly at the focus of the object-glass. The small tube which carries the slit slides into the body of the telescope, but a rack-and-pinion motion would be preferable, so that when the focus is changed the slit will necessarily remain vertical. The rays of light diverging from the slit and rendered parallel by the object-glass of the collimator next pass through a series of prisms adjusted around a conical wheel, which will be soon described, and are then received by the telescope B. The spectrum, which is formed at the focus of the object-glass of this telescope, is examined with eyepieces of different magnifying powers in the usual way. The object-glasses of the telescopes are $2\frac{1}{4}$ inches in diameter, and have a focal length of $15\frac{1}{2}$ inches. They rest in Y's, and are provided with spirit levels and adjusting-screws. The frames which hold the telescopes are supported on pivots turning in sockets at the ends of two arms connected with the body of the instrument, and may be clamped in any position. The arm which carries the collimator is permanently attached to the main iron plate, but the arm which carries the observing-telescope may be moved around the plate.

The details of the construction are shown in fig. 2, which is a section made through one of the legs of the tripod and the moveable arm, the telescope with its frame and pivot having first been removed from the socket. (This figure, as well as fig. 4,

were drawn to the scale of one inch to a foot.) The parts are as follows:—H is an iron tripod with levelling-screws; G is a

Fig. 2.



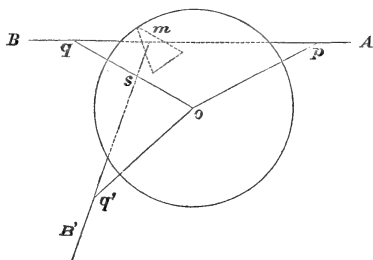
hollow mahogany column with a conical cavity at the top; F is an iron cone which rests in the conical cavity, supporting the whole body of the instrument, and connected by a long iron rod with a clamping-screw beneath the tripod. By means of this arrangement the instrument may be turned as a whole in the horizontal plane and the collimator directed to the source of light. Above the iron cone, and fastened to it securely, is the main circular plate of the instrument, which is 18 inches in diameter and $\frac{1}{2}$ an inch in thickness. Near the outer edge of this plate is inserted a band of silver, which is graduated to $10''$ of arc. On the under part of the plate there is a neck, and at the centre of the upper surface a socket, which are accurately centred with each other and with the graduated limb. Around the neck at E moves an iron collar, to which is attached the arm bearing the observing-telescope. This moves, therefore, concentric with the graduated limb, and bears a vernier, by which the angular motion may be determined, reading to $10''$. In the socket of the first plate rests the pivot of a second plate, C, which turns on the first and supports the prisms with the adjusting-wheel A. The diameter of the upper plate is an inch less than that of the lower plate, so as to expose the graduated arc near the outer edge of the latter; and its upper surface is as flat and even as possible. Rising from the centre of the upper

plate there is a tall screw pivot of iron, B, on which turns a conical wheel, made also of iron. By this motion the wheel may be either raised or lowered. This wheel is an essential portion of the instrument, and we will next consider the theory of its use.

In the ordinary method of measuring the angle of minimum deviation with a Babinet's goniometer, the prism is placed on a revolving plate at the centre of the graduated circle, and so adjusted that the edge of the refracting angle shall be perpendicular to the plane of the circle, and its bisectrix parallel with a diameter of the circle. The axis of the collimator and observing-telescope, moreover, being parallel with a diameter of the circle, it is evident that, when the prism and telescope are turned into the position of minimum deviation, the vertex of the angle of minimum deviation will coincide with the centre of the circle, and hence the arc intercepted between two radii of the circle parallel to the axes of the two telescopes will be the measure of the angle required. This angle is practically measured by first bringing into line of collimation the observing-telescope and collimator, so that the image of the slit at the end of the collimator coincides with the vertical wire in the eyepiece of the telescope. The position of the vernier on the graduated arc is now noted. Then, having adjusted the prism, both the prism and the telescope are turned to the position of minimum deviation, and a coincidence established between the vertical wire and one of the lines of the spectrum. The vernier is now again read, and the difference between the two readings is the angle of minimum deviation for the ray corresponding to that line.

It will be obvious, however, from fig. 3, that it is not necessary for the accuracy of this measure, either that the prism should be placed at the centre of the circle, or that the axis of the telescope should be parallel to one of its radii. If only the bisectrix of the refracting angle passes through the centre of the circle, the prism may be placed on the outer rim of the plate; and if only the radial arm, which carries the observing-telescope, moves concentric with the graduated arc, the axis of the telescopes themselves may make any angle with the radius whatever. Let Op and Oq be two radii of the graduated circle. Let Ap and Bq represent the axes of the two telescopes in col-

Fig. 3.



limation, and making any undetermined angles with the two radii. Place now a prism at m , and turn the radial arm Oq into the position Oq' , but without changing the inclination of the axis of the telescope to the arm, and let BmB' be the angle of minimum deviation. Since now the two triangles qsm and $q'so$ are similar, it is evident that the angle BmB' is equal to the angle qoq' , and is therefore measured by the arc intercepted between the radii Oq and Oq' .

In order now to apply this principle in the spectroscope, the glass prisms were mounted permanently in brass frames. The frames rest on three brass pins, which were adjusted by filing until the refracting edge of the prism was perpendicular to the iron plate C (fig. 2). Two brass pins were also attached to the back of each prism, and the lengths of these so adjusted that, when the prisms were pushed against the conical wheel, the bisectrix of the refracting angle should coincide with a radius of the wheel. The last adjustment was made with the aid of a gauge cut from a sheet of tinned iron, fitting at the same time the periphery of the wheel and the face of the prism, which was applied alternately on either side. The angle of minimum deviation of the ray D was then measured for each prism in the following way.

The prism having been placed on the plate with the pins applied to the periphery of the wheel, the collimator was turned on its pivot, and at the same time the plate C turned on its centre, until, on applying the eye at the open slit and looking through the object-glass towards the prism, the further back edge of the prism, seen through the glass of the prism, appeared to coincide with the nearer back edge seen directly. When this is the case, it is evident that the rays of light which reach the eye from the further back edge of the prism must pass through the glass parallel to the back edge of the prism, or, what amounts to the same thing, perpendicular to the bisectrix of the refracting angle; and when the light passes in this way, the prism is at the angle of minimum deviation. When the prism was thus placed, the collimator was turned slightly on its pivot until the axis of the telescope prolonged passed through the centre of the prism-face, and was then securely clamped in this position. This adjustment may be made with sufficient accuracy by the unassisted eye. The prism having now been turned on one side, the arm of the observing-telescope was turned on its centre, and at the same time the telescope turned on its pivot until it came into exact collimation with the collimator. In order to facilitate this adjustment, the telescopes are provided with caps which cover the object-glasses of the telescopes with the exception of a narrow vertical slit at the centre. The pivot of the observing-telescope was next clamped,

and, the caps having been removed, the image of the slit seen through the observing-telescope was brought into exact coincidence with the vertical wire, and the position of the vernier noted. The prism was now brought back to its place by turning the upper plate of the instrument, and the observing-telescope also turned until the position of minimum deviation for the ray D was attained, and this well-known double line brought to coincide with the vertical wire. The limb was then again read; and the difference of the two readings gave the angle of minimum deviation for the prism.

In order to show that this method of measurement is perfectly accurate, we give below the angles of minimum deviation of the nine prisms of the Cambridge spectroscope, measured as just described, and in a parallel column the same angles measured in the old way with the prisms at the centre of the plate. It will be seen that the differences are insignificant, and within the limits of error of observation.

	Measured at centre of plate.	Measured on edge of plate.	
No. 1.	29° 31' 10"	29° 31' 10"	
No. 2.	29 29 10	29 29 10	
No. 3.	29 28 10	29 28 10	
No. 4.	29 37 0	29 36 40	-20
No. 5.	29 28 30	29 28 40	+10
No. 6.	29 36 30	29 36 10	-20
No. 7.	29 28 10	29 28 10	
No. 8.	29 29 30	29 29 40	+10
No. 9.	29 28 40	29 29 40	
	267 37 50	267 37 30	-20

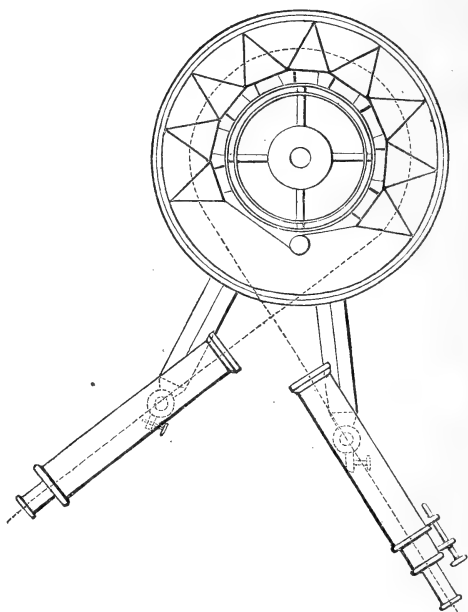
Although the adjustments required may appear complicated, they can be made in far less time than it has taken to describe the method.

It is a well-known fact that when a beam of homogeneous light passes through a prism at the angle of minimum deviation, the incident and emergent pencils make the same angle with the faces of the prism at which they respectively enter and leave the glass. Hence a second prism, like the first, stands in the same relation to the emerging beam in which the first stands to the incident beam. If, then, after the first prism has been adjusted at the angle of minimum deviation, a second prism be applied against the wheel at the side of the first, by moving the prism slightly to one side or the other it will be easy to find a position in which this prism also is at the angle of minimum deviation,

moving, of course, the prism on the plate without disturbing the position of the plate itself. In like manner, other prisms may be added until the required number is obtained. (In the Cambridge spectroscope there are nine glass prisms of 45° , as shown in fig. 4.) This adjustment has only to be made, however,

once for all, since, when the prisms are adjusted, they are fastened to a very thin flexible brass ribbon, which, passing through a box at the back of each prism, is there secured by clamping - screws. The ends of this ribbon, moreover, are attached to a small brass drum, which, being wound up by an ordinary clock-spring, draws the chain of prisms tightly around the conical wheel, and keeps them always in place. By tracing the path of a ray of homogeneous light through

Fig. 4.



a series of similar prisms, as is shown in fig. 4, it will be found that the path of the ray within the prisms is always a tangent to the same circle, provided that it passes through all under the conditions of least deviation. Assuming, then, that the distances between the prisms are invariable, as they must be when the prisms are fastened to a brass ribbon as just described, it will be evident, from a moment's reflection, that, the greater the refrangibility of the given ray, the less must be the diameter of the circle around which the prisms should be arranged in order that the ray may pass under the required conditions; and knowing the dimensions of the prisms as well as the index of refraction and dispersive power of the glass, it is easy to calculate approximately what the diameter should be in a given case. The dimensions of the conical wheel A, fig. 2, were thus determined—the largest diameter, $9\frac{1}{2}$ inches, corresponding to the

extreme red, and the smallest diameter, $8\frac{1}{2}$ inches, corresponding to the extreme violet rays of the solar spectrum. In order to facilitate the adjustment, a series of lines were engraved encircling the wheel at equal distances from each other, and numbered from 1 to 12.

Having described the construction of the instrument, it will now be easy to understand the method of using it. Let us suppose that the object is to measure the angle of minimum deviation of the blue ray of the strontium spectrum. By examining any chart of the spectra of the chemical elements, it will be found that this line is situated roughly at somewhat less than two-thirds of the distance from A to H. If, then, we turn the conical wheel until the pins of the prisms all rest against the line marked 7, we shall have approximately the true position. We then adjust the collimator with reference to the first prism exactly as before described for a single prism. Turning then the upper plate so as to remove the prisms out of range, we bring the observing-telescope into collimation with the collimator, as also before described, when, on reading off the limb, we have the starting-point for our measure. We next turn the plate and move the telescope until the spectrum appears in the field, and carefully bring the blue line to coincide with the vertical wire at the position of minimum deviation. We now raise or lower the conical wheel and notice if in this way the angular deviation is diminished, and leave the wheel in the position where the minimum is reached. It now only remains to again read the limb, when the difference of the two readings subtracted from 360° will give the angular deviation required.

When near the position of minimum deviation, a large motion of the conical wheel produces only a slight motion of the image; so that, after a table has been made giving the position of the wheel for a few of the marked lines of the spectrum, it is always possible to bring the wheel at starting to the desired point. Moreover the fact that, when near the position of minimum deviation, the position of the image is affected so slightly by a small change in the position of the prisms renders it possible to make all the adjustments required with exceeding rapidity and accuracy.

In order to test the accuracy of our method, we have made several determinations of the minimum deviation of the line D; and although between each determination the whole apparatus was completely dismantled, the value obtained was in all cases $267^\circ 37' 50''^*$. It will be remembered that the sum of the angles measured on each prism separately at the centre of the plate, as

* As the mean of the two lines.

given on page 115, is precisely the same ($267^{\circ} 37' 50''$); and the sum of those measured on the edge of the plate ($267^{\circ} 37' 30''$) only differs from this by $20''$.

When it is not important to use absolutely the whole aperture of the prisms, it is not necessary to change the position of the collimator in passing from one part of the spectrum to another. If we adjust the collimator as above described, when the prisms rest against the middle circle on the wheel, the whole spectrum can be passed under review with great rapidity without any further change of the collimator, and each point seen under the condition of minimum deviation. When, however, on account of the feebleness of the light, it is important to use the whole aperture of the prisms, a slight gain can be obtained by readjusting the collimator at the extreme points of the spectrum. In making the measurements described in this paper, an important advantage is gained by keeping the position of the collimator fixed; for if its position is changed, the point to which the angular measurements are referred is changed also, and must be determined anew. If, however, the arms to which the telescopes are attached are so arranged with a sliding motion that both the collimator and the observing-telescope may be moved parallel to themselves without altering their relative angular position, the whole aperture of the prisms may at any time be used, and nevertheless all the measurements referred to the same point on the graduated limb.

Besides the set of glass prisms, the Cambridge spectroscope is also provided with a set of sulphide-of-carbon prisms. They are mounted on a separate plate with a separate wheel of the proper dimensions, and are arranged in all respects like the glass prisms above described. The plates are provided with handles, so that one battery of prisms may be quickly lifted off from the instrument and the other put in its place. But although the liquid prisms are valuable on account of their great dispersive power in bringing out faint lines, especially in the more luminous portions of the spectrum, yet their use is very restricted. One difficulty arises from the immensely rapid change of the index of refraction of sulphide of carbon with the slightest change of temperature. We have noticed within half an hour on a summer's day a change of $8'$ in the angle of minimum deviation of a single sulphide-of-carbon prism of 45° , although the temperature of the room had only in the meantime changed eight-tenths of a Centigrade degree. The temperature of the prism undoubtedly changed much more than this; but when we remember that the variation thus produced would amount to over one degree for the nine prisms, it will be seen that they must be useless for any purposes of direct measurement. Again, sulphide

of carbon is far less transparent than glass to the more refrangible rays of the spectrum ; and lastly, a slight change of temperature in the observing-chamber produces at once currents in the liquid which are fatal to good definition. Nevertheless, under the best conditions, we have found that sulphide-of-carbon prisms define as well or even better than glass. The instrument described in this paper, with the exception of the glass prisms, was made by Messrs. Clark and Sons of Cambridgeport ; and we would here especially express our indebtedness to Mr. George Clark for his great ingenuity in planning and executing the mechanical details.

XVI. *On Regelation.* By J. GILL, Esq.*

PROFESSOR TYNDALL'S ingenious explanation of glacier-motion, in referring it to fracture and regelation of the ice, is satisfactorily proved by his experiments of moulding solid ice into the most varied forms by pressure only, and it seems to be sanctioned by the opinions of Helmholtz and De la Rive ; but various opinions still prevail as to the immediate cause of regelation, or the manner in which the action takes place. Tyndall imagines that the particles at the surface of a mass of ice, being free on one side, have greater liberty of motion than the particles in the interior of the mass, and consequently will melt at a lower temperature. He proved by experiment that in ice containing air and water-cells, the ice in the interior of the mass can become liquid by heat which is conducted through the solid substance without dissolving it, but which melts the ice forming the walls of these little chambers where the particles are free on one side, and therefore can liquefy more easily. In this respect the solid particles forming the sides of the cells are in a condition similar to that of the particles on the outside of the block ; and as regards temperature, the interior of the solid mass is supposed to be virtually colder than the surface ; that is to say, its particles have less liberty of motion, consequently less motion. When two pieces of ice at 32° with moistened surfaces are placed in contact with each other, their touching portions are, as it were, transferred to the centre of the mass. "Before being brought together the surfaces had the motion of liquidity, but the interior of the ice had not this motion ; and as equilibrium will soon set in between the masses on each side of the liquid film and the film itself, the film will be reduced to a state of motion inconsistent with liquidity. *In other words, it will be frozen, and will cement the two surfaces of ice between which it is enclosed.*"—Tyndall.

* Communicated by the Author.

Still the difficulty remains of the disposal of the heat of liquidity of the frozen water. Professor Tyndall is no doubt right in assuming that the interior portions of a mass of ice require a higher temperature to liquefy than the surfaces, and consequently that they may conduct and transmit heat without melting. But as this very fact supposes them to be less susceptible of individual molecular motion, it is difficult to imagine that they can absorb or retain heat, unless from a source of appreciably higher temperature—that is to say, a temperature above the margin which distinguishes the capability of liquefaction between the free and the confined particles, no cause of which is given; and it is not supposed that the film of water between the appressed surfaces is subject to any other cause of freezing than the mere sympathetic action tending to induce synchronism of molecular motion, or identity of condition in the neighbouring particles.

The explanation given by Professor James Thomson is based on his interesting discovery of the lowering of the freezing-point of water by compression, and is briefly as follows:—"The two pieces of ice, on being pressed together at their points of contact, will at that place, in virtue of the pressure, be in part liquefied and reduced in temperature, and the cold evolved in their liquefaction will cause some of the liquid film intervening between the two masses to freeze." Of course a sufficient cause of the pressure should be shown; and moreover it must be supposed that the liquid film between the appressed solid surfaces is free (or comparatively free) from pressure. With this condition the explanation seems satisfactory; and Helmholtz supposes this condition to obtain in the case of glacier-motion, as the water of the compressed ice can escape through fissures. This seems questionable as a general fact; but, supposing that in general the water is confined and retained in the internal fissures, the phenomena might still be explained by supposing the degree of pressure to be continually varying in any given spot in the mass from the extensive process of internal fracture and change of shape always going on in the glacier from its descending motion through a mould of very irregular conformation. When a block of ice is compressed, layers of liquid water are formed in the substance of the solid at right angles to the direction of pressure. The temperature of the ice must be lowered by a quantity corresponding to the heat of liquefaction of the water produced; and the water, being under the same pressure, remains liquid in contact with the cooled ice, because its freezing-point is lowered by the pressure. On the removal of the pressure (supposing the ice to return to its former dimensions, not by elasticity, but by a new mechanical arrangement of its parts), the water freezes

again and throws out the heat of liquidity of the frozen particles into the surrounding ice.

But in the case of instantaneous regelation in fragments of ice merely touching each other, and even when floating in warm water, there does not seem to be sufficient cause of the pressure required by J. Thomson's explanation, even keeping in mind the fact pointed out by Helmholtz, that the pressure is not equally distributed over the whole of the two appressed surfaces, but is concentrated on a few points of contact. Moreover, in the case of wet surfaces, or when the fragments of ice are floating in warm water, it is not easy to imagine how any pressure acting on the pieces of ice should not at the same time act on the films of water interposed between the surfaces of contact.

It must therefore be allowed that, in such cases of regelation, a sufficient cause of pressure is not apparent; yet Helmholtz insists on the probability of pressure being the proximate cause of regelation. He says, "I find the strength and rapidity of the union of the pieces of ice in such complete correspondence with the amount of pressure employed, that I cannot doubt that the pressure is actually the sufficient cause of the union." On this, Tyndall remarks that Faraday's contact-action would also increase under pressure, from the greater extent of surface which would be brought into play; and he insists on the difficulty of imagining any sufficient cause of pressure in the regelation of fragments of ice floating in warm water which freeze together at their points of contact "in a moment"—convex surfaces freeze together—"virtual points that touch each other, clasped all round by the warm liquid which is rapidly dissolving them as they approach each other." (Phil. Mag., December 1865.)

Evidently the chief difficulty in the inquiry is to explain the existence of a sufficient cause of pressure on the surfaces of solid contact, while at the same time the liquid films enclosed between these surfaces should be free from pressure; and as none of the explanations hitherto given seem to satisfy these conditions, the following hypothesis is offered as apparently meeting the physical requirements of the case.

It seems probable that all bodies are continually sending off particles of their substance into the surrounding atmosphere. Ice certainly is known to evaporate at all temperatures, from the freezing-point to the lowest temperature which has been observed; and the "disgregation" or "metamorphic" action constituting the phenomenon may be supposed to involve an expenditure of heat (or other energy) equal to convert the particles of solid ice into particles of gaseous steam, though we should imagine the motion of the disgregated particles to be straight-line motion, or the true motion of free gaseous particles in space, the

distance of the particles from each other being too great to admit of any appreciable mutual interaction between them. The evaporating surface of the ice is therefore a field of active molecular operation; in whatever way heat may be getting in, it is certainly going out with each disgregated or metamorphosed particle which flies off from the surface of the solid.

Water, within a small range above its freezing-point, expands with cold; and as it gives this marked evidence of being in *an exceptional state of thermic susceptibility* under these circumstances, we might naturally suppose that in this condition its thermic properties generally were in an inverted state. It is certain, at least, that between $39\frac{1}{2}^{\circ}$ and 32° water contracts with heat. Very thin liquid films on the surfaces of ice may be supposed to be at the maximum limit of this inverted state of thermometric action, and would certainly contract on being slightly heated.

When two pieces of ice are brought together, we should imagine that the motion of translation of the escaping disgregated particles being mutually stopped, local heat must be the result. In the case of two fragments of wet ice touching each other without any appreciable pressure artificially applied, the actual points of solid contact may be supposed to be very minute, with comparatively large surfaces of water-films between; and the smallest extent of "spherical surfaces" which we can imagine practically to come to touch each other should, when conceived of molecularly, give the idea of considerable space, including many of the "virtual points that touch other," as described by Tyndall, with very shallow water-spaces between. Now the sudden generation of heat, developed, between the pieces of ice in the act of contact, from the arrested motion of translation of the disgregated particles flying off from the surfaces, must raise the temperature of the surfaces where it acts. The sudden melting of the projecting prominences of solid ice would probably be an action of very limited extent compared with the heating of the intervening liquid films, which, shut in from free communication with the exterior, and instantly contracting with the increase of temperature, would cause locally a partial vacuum between the contiguous solid points of support, while the atmospheric pressure acting on the back of each piece of ice might cause a very considerable amount of pressure on the solid points, in addition, perhaps, to a kind of capillary attraction existing between surfaces in contact.

The effects of the pressure thus brought into action may be conceived of as results of the interesting fact of the lowering of the freezing-point of water by compression. Helmholtz (quoted by Tyndall) says, "The pressed ice will become colder by a

quantity corresponding to the lowering of its freezing-point by the pressure. But the freezing-point of the uncompressed water is not lowered. [This is said of the glacier-water supposed to be free to escape through fissures; but it applies here, as the cause assigned to the pressure on the ice is the shrinking of the intervening water.] Here, then, we have ice colder than 0° C. in contact with water at 0° C. [the water of liquefaction]. The consequence is, that round the place of pressure the water will freeze and form new ice, while, on the other hand, a portion of the compressed ice continues to be melted." The water of liquefaction would in the first place form zones round the solid points of contact, displacing laterally the surrounding water of the liquid films, while the particles of ice simultaneously forming round the solid prominences would act as wedges or props to compensate the lowering of the points by liquefaction, and so maintain the original distance of the surfaces (approximately), and consequently the partial vacuum originally formed by the shrinking of the liquid films. Meanwhile the heat thrown out by the forming ice, and the heat of the enclosed films which are at a temperature above that of the surrounding ice, would pass into the solid mass,—perhaps quite through it without causing liquefaction, according to Tyndall's idea; but at least, in the case under consideration, the heat should naturally pass from the water into the ice, because the temperature of the water should be sensibly higher.

When fragments of ice are floating in warm water with the appressed surfaces submerged, we must suppose that before contact took place the surfaces were melting and sending off liquid particles at or very little above 32° at the moment of their separation from the solid mass, which particles would be floating away with a certain amount of *vis viva* in their motion of translation. On the contact of the two pieces of ice this motion would be stopped and changed into heat, which would act locally on the liquid film formed by the arrested particles themselves, converting their previous motion of translation into heat-motion; and hence the shrinking of the liquid films, and consequent pressure on the solid points of contact required to explain the fact of regelation.

Palermo, December 24, 1865.

XVII. *Studies on Gases.*

By Dr. H. W. SCHROEDER VAN DER KOLK*.

§ I. *Introduction.*

IN so far as the principle of the mechanical theory of heat—that the condition of a body only depends on two given magnitudes—can be considered to hold good, the formula $pv = k\tau$, where p is the pressure, v the volume, and τ the absolute temperature, may be regarded as valid for all bodies. In general, k is then a variable magnitude dependent on temperature and pressure; if it is made constant, the formula is obtained of the body which is called an ideal gas. Actual gases approximate to this condition; the changes which temperature and pressure here produce in the value of k are very small. For some gases, especially for hydrogen, air, carbonic acid, and nitrogen, Regnault's determinations render it possible to determine more accurately the changes in k . From his experiments in reference to Mariotte's law, I have calculated for these gases the formula which indicates the connexion between k and p at the mean temperature of 4° , as I have already communicated in a former memoir†. From the coefficients of expansion between 0° and 100° under different pressures, the same formula may be calculated for 100° . It was found that in the case of hydrogen, at any rate within the limits of the accuracy of Regnault's experiments, k does not alter with the temperature, while in the case of the other gases it is a function of both variables. Other determinations of Regnault rendered it possible to test the accuracy of the formula thus obtained.

If the principle that the condition of a body is determined by means of two magnitudes, and therefore by means of the changes in k , be compared with Regnault's result, that the specific heat of water and of air scarcely changes at all with the temperature, while that of carbonic acid, in which k undergoes a far greater alteration, changes considerably, it is natural to investigate whether these changes of the specific heat are not connected with changes in k .

This investigation is connected with an accurate determination of the mechanical equivalent of heat. Hence I repeated the calculation of Moll and van Beek's experiments on the velocity of sound, in order to have a more accurate determination of the coefficient $\frac{c_1}{c}$ ‡. The ordinary formula for calculating the con-

* Translated from Poggendorff's *Annalen*, vol. cxxvi. p. 333.† Poggendorff's *Annalen*, vol. cxvi. p. 429.

‡ In this paper I have adhered to Clausius's proposal to designate the

stant J^* , or the mechanical equivalent of heat, is then $(c_1 - c)J = \frac{p\alpha}{b}$, in which α is the coefficient of expansion for 1° , b the density, and p the pressure. This formula could not, however, be used in the present case. For, in the first place, an ideal gas is supposed in the formula, whereas the experiments refer to air. The changes which pressure and temperature produce in the value of k —that is, the deviations from Mariotte and Gay-Lussac's law—had to be taken into account. In the second place, the formula was not applicable, because the first member gives the difference of the specific heat, the second the work done, and because both these magnitudes are only equal in case Mayer's assumption is applicable, that the gas performs no internal work, which with air is by no means the case. The formula

$$c_1 - c = \frac{\tau}{J \left(\frac{d\tau}{dp} \right) \left(\frac{d\tau}{dv} \right)} \dagger$$

is, on the other hand, universally valid, and therefore can be used. For this case the differential quotients $\frac{d\tau}{dp}$ and $\frac{d\tau}{dv}$ were changed into a function of $\frac{dk}{dp}$ and $\frac{dk}{d\tau}$, by which is obtained the formula

$$c_1 - c = \frac{\left(k + \tau \frac{dk}{d\tau} \right)^2}{J \left(k - p \frac{dk}{dp} \right)}.$$

For atmospheric air, c_1 is known from Regnault's experiments, $\frac{c_1}{c}$ from the velocity of sound, k and $\frac{dk}{dp}$ from the formulæ previously obtained. The determination of $\frac{dk}{d\tau}$ presented, however, some difficulty. The values of k at 4° and 100° are indeed known; but the supposition that k changes regularly with the temperature is untenable, because all gases at lower pressure and higher temperature approximate to ideal gases, which in this case would be impossible. But the divergent numbers

specific heat under constant volume by the letter c , and under constant pressure by c_1 . Clausius, 'Memoirs on the Mechanical Theory of Heat,' p. 291.

* I have here kept to the English notation, as the letter Λ occurs further on with a different meaning.

† Zeuner, *Grundzüge der mechanischen Wärmetheorie*, p. 177.

obtained on calculation showed the inadmissibility of this assumption.

Hence the entire calculation reverted to an accurate measurement of $\frac{dk}{d\tau}$ at various temperatures. If, now, k were known for three different temperatures, $\frac{dk}{d\tau}$ might be obtained with sufficient accuracy by means of an interpolation formula; Regnault's determinations give, however, only two values of k , at 4° and 100° . Hence a third value of k had to be determined in an indirect manner, which calculation is given in § III. By means of this value J , or the mechanical equivalent of heat, could easily be determined; and the same formula could inversely be used for determining the difference of the two specific heats for other gases under different pressure and different temperature.

As now the external work of the gases, performed in expansion during heating under constant pressure, was known, the internal work in expansion could also be determined; in other words, it could be determined how much more energy was accumulated in a given quantity of a gas of constant temperature with a greater volume than with a smaller one.

These different subjects are more minutely discussed in the following paragraphs.

§ II. *On the Determination of k .*

The results previously obtained may, in so far as they are applied in the sequel, be here briefly repeated.

The coefficient of expansion under constant pressure changes in all cases with the pressure, except in the case of hydrogen; in this case Regnault found, for the barometric height h ,

$$\begin{aligned} h &= 0.760, & 0.36613, \\ h &= 2.545, & 0.36616, \end{aligned}$$

values, therefore, which are to be considered equal.

Hydrogen is alone in this respect. If, therefore, different volumes of hydrogen, which are under different but constant pressures, are allowed to expand, the proportion of the volumes at 0° and 100° will be the same in each case.

With an ideal gas, that is, one in which k is constant, the same is the case; for from the formulæ

$$\begin{aligned} pv &= k\tau, \\ pv_1 &= k\tau_1, \end{aligned}$$

in which p is the pressure in kilogrammes on the square metre, we get by division

$$\frac{v}{v_1} = \frac{\tau}{\tau_1}.$$

The proportion of the volumes of an ideal gas is thus like that of hydrogen.

Thus, in the case of hydrogen as in the case of an ideal gas, k must be regarded as independent of the temperature; with hydrogen, indeed, k changes with the pressure; but this is without influence in the present case, in which the pressure is constant. For measuring the temperatures, let us suppose a thermometer filled with hydrogen, in which the gas expands under constant pressure, which is first immersed in boiling water and then in melting snow, and between these points is divided into 100 equal parts. Each part will represent a degree of the scale taken in the sequel as the basis in calculation.

It is easily seen that the indications of this thermometer will be perfectly like those of one filled with an ideal gas.

Taking the coefficient of expansion $= 0.3661$, we obtain for the absolute zero $-273^{\circ}15$.

For calculating k under different pressures, there was obtained in the case of hydrogen

$$k_h = \{1 + A(h-1) + B(h-1)^2\} K,$$

in which

$$A = 0.00038969,$$

$$B = 0.000039831,$$

$$K = 422.337.$$

The volume of 1 kilog. at 0° and 0.760 metre pressure at Paris

$$= 11.16346 \text{ cubic metres.}$$

In so far as k , in virtue of Regnault's results, can be considered to be independent of the temperature, this formula is valid for all temperatures.

In the case of air at 4° , the formula was obtained,

$$k_h^4 = [1 - A(h-1) + B(h-1)^2] K,$$

$$A = 0.00124351,$$

$$B = 0.0000229842,$$

$$K = 29.2443*.$$

The volume of 1 kilog. at 0° C. and 0.760 metre pressure $= 0.773283$ cubic metre. At 100° the formula was obtained,

$$k_h^{100} = [1.00062 - 0.000111(h-1)] K.$$

* This value, as well as that for carbonic acid, is somewhat different from the one already given, as will be shown in the next paragraph.

For carbonic acid at 3° , was obtained

$$k_h^3 = [1 - A(h-1) - B(h-1)^2] K,$$

$$A = 0.008567,$$

$$B = 0.000111,$$

$$K = 19.0949.$$

Volume of 1 kilog. at 0° and 0.760 metre pressure = 0.505711 cubic metre. At 100° , on the contrary,

$$k_h^{100} = [1 - A(h-1) - B(h-1)^2] 1.00391 K,$$

$$A = 0.002465,$$

$$B = 0.000275.$$

h always denotes the barometric height in metres; and as p gives the pressure in kilogrammes, in all cases $p = 13595h$. Péclet, as is well known, has expressed the opinion that the deviations from Mariotte and Gay-Lussac's law might be caused by adhesion of the gases to the glass sides. Such an adhesion undoubtedly takes place; the question is, whether it is of appreciable influence on the formulæ obtained. In the first place, the assumption that the deviations arise only from adhesion is very improbable, as with vapours in the neighbourhood of the points of condensation deviations do undoubtedly occur, and between gases and vapours a quantitative and not qualitative difference is to be assumed. It follows, however, from Regnault's weighings that the errors are as good as imperceptible. If the weight of a gas contained in a globe at a definite temperature is determined for two different pressures, the second value may be calculated from the first by means of the formulæ of interpolation obtained. The condensed gaseous layer only affects the weighing, and not the measurement of the pressure; for this always corresponds to the pressure of the uncondensed gaseous mass. Now Regnault found that the weight of a definite volume of carbonic acid at $100^{\circ}.01$ and 755.65 millims. pressure was 14.19 grammes, while at the same temperature, but under a pressure of 338.39 millims., the weight was 6.3587 grammes. Now if this value be calculated from the first determination by means of the formulæ of interpolation obtained, which are only deduced from the pressures, we get 6.3585 grms., which is almost exactly equal to the value obtained directly*.

At a lower pressure the weighings at 0° exhibit a small difference, which only amounts, however, to $\frac{1}{3000}$ of the magnitude to be determined: this may arise from the fact that the formula of interpolation developed from higher pressures differs a little in the case of a lower pressure of 0.2 metre.

* Poggendorff's *Annalen*, vol. cxvi. p. 444.

That the influence of adhesion on the formulæ obtained is inappreciable, follows also from the examination of the law of volumes given in the fourth section.

§ III. Determination of $\frac{dk}{d\tau}$.

Regnault's determinations of Mariotte's law and of the coefficients of expansion give, as already observed, values only for the two temperatures 4° and 100° . But Regnault has compared with each other thermometers filled with different gases, and has found that two thermometers filled with air and hydrogen were quite parallel if, in both, the fixed points for 0° and 100° were first determined. The temperatures were inferred from the increase of pressure under constant volume. He extended these investigations to between -87° and 300° , between which limits the thermometers were parallel. At the lowest temperature the difference was only $0^\circ.2^*$, and in this calculation quite to be neglected. Supposing, now, three determinations of air and hydrogen at 0° , 100° , and n degrees, the condition of these gases in these three cases will be given by the formula

Air.	Hydrogen.
$p_1 v = k_1 273,$	$p_I v = k_I 273,$
$p_2 v = k_2 373,$	$p_{II} v = k_{II} 373,$
$p_3 v = k_3 (273 + n),$	$p_{III} v = k_{III} (273 + n) ;$

and as Regnault considered the temperatures proportional to the pressures, the proper expression of his result is

$$p_3 - p_1 : p_2 - p_1 = p_{III} - p_I : p_{II} - p_I,$$

or

$$\begin{aligned} k_3(273 + n) - 273k_1 : 373k_2 - 273k_1 \\ = k_{III}(273 + n) - 273k_I : 373k_{II} - 273k_I. \end{aligned}$$

Since k may, in the case of hydrogen, be considered to be independent of the temperature, k_I , k_{II} , and k_{III} may be calculated by means of the formula given for hydrogen in the preceding section; for the pressure may in each case be calculated with sufficient accuracy from the temperatures, which in this case is sufficient, for the pressure is only implicitly contained in the value of k . For air, k_1 and k_2 may be calculated by means of the formulæ already found, and hence k_3 alone remains to be determined. In this way, for instance, is found the value of k for 50° and 1 metre pressure.

* Daguin, *Traité de Physique*, vol. i. p. 833.

Temp. °	Pressure.	Hydrogen.	Air.
$t = 4,$	$h_1 = \frac{277}{323} = 0.858,$	$k_I = 0.999945,$	$k_1 = 1.000177,$
$t = 100,$	$h_2 = \frac{277}{323} = 1.155,$	$k_{II} = 1.000069,$	$k_2 = 1.000601.$
$t = 50,$	$h_3 = 1$	$k_{III} = 1.000000,$	

273° , as being sufficiently accurate, is here taken instead of $273^\circ.15$.

We have now the proportion

$$323k_3 - 277 \times 1.000177 : 373 \times 1.000601 - 277 \times 1.000177 \\ = 323 - 277 \times 0.999945 : 373 \times 1.000069 - 277 \times 0.999945,$$

from which is obtained

$$k_3 = 1.000403 \text{ K.}$$

The above values of k ought properly to be multiplied by K , which is constant for each gas. But as we have here only to do with changes of k , I have omitted to do so; K , in fact, entirely disappears from the proportion.

For calculating the empirical formula I calculated the values for -87° . For a pressure of 2 metres we have, then,

Temp.	Absolute temp.	Pressure.	Hydrogen.	Air.
$-87^\circ,$	$186,$	2	$k_I = 1.000429,$	
4,	277,	$\frac{277}{186} = 2.980,$	$k_{II} = 1.000928,$	$k_2 = 0.997627,$
100,	373,	$\frac{373}{186} = 3.990,$	$k_{III} = 1.001417,$	$k_3 = 1.000286,$

and the proportion

$$373k_3 - 277k_2 : 277k_2 - 186k_1 \\ = 373k_{III} - 277k_{II} : 277k_{II} - 186k_I.$$

from which k_1 can be calculated.

Although it follows from Regnault's observations that, in the case of hydrogen, k does not change with the temperature between 0° and 100° , this is of course only considered to be proved within the limits of the accuracy of these experiments. It is not very probable that k is absolutely independent of t ; for at no temperature would hydrogen then approximate to the ideal gas, as do other gases: the question is, whether an influence of this kind is still imperceptible between such wide limits as -87° and 100° . If, however, k_I is too great, k_{III} must, owing to continuity, be too small at 100° , since k_{II} must be considered correct; and, inversely, the last two members of the above proportion will then be either both too large or too small; and as 4° is almost halfway between -87° and 100° , and the changes are in any case very small, the ratio will not be perceptibly altered.

The following results are thus obtained:—

Table of the Values of k for Air.

Temperature.	Pressure.		
	0·76.	1 metre.	2 metres.
—87°	0·99862	0·99749	0·99300
4	1·00030	1·00000	0·99878
50	1·00055	1·00040	0·99986
100	1·00064	1·00062	1·00051

The differences increase here regularly with the pressure and with sinking temperature.

From the values at -87° , 4° , and 100° , three formulæ of interpolation were calculated for the three pressures. There was obtained,

$$\text{at } 0^{\text{m}}\cdot 76 \quad k = \left\{ 1\cdot 00112 - \frac{0\cdot 11097}{131\cdot 4 + t} \right\} \text{K},$$

$$\text{at } 1^{\text{m}} \quad k = \left\{ 1\cdot 00158 - \frac{0\cdot 23366}{144\cdot 17 + t} \right\} \text{K},$$

$$\text{at } 2^{\text{m}} \quad k = \left\{ 1\cdot 00348 - \frac{0\cdot 77662}{161\cdot 07 + t} \right\} \text{K};$$

t is here the temperature on the Celsius scale.

As a control, the value of k for 50° was calculated. There was found—

Pressure.	From the formula.	By direct observation.	Difference.
0 ^m ·76	1·00051	1·00055	4
1	1·00038	1·00040	2
2	0·99980	0·99986	6

Hence between the above-mentioned limits of temperature the formulæ may be considered to be sufficiently accurate.

By differentiating this formula, the values of $\frac{dk}{dt}$ are obtained.

At 0·76 metre pressure, for instance,

$$\frac{dk}{dt} = \frac{0\cdot 11097 \text{ K}}{(131\cdot 4 + t)^2},$$

and thus at 4°C. ,

$$\frac{dk}{dt} = 0\cdot 0000060526 \text{ K.}$$

At 200° somewhat divergent results are obtained, perhaps because the formula of hydrogen is too inaccurate at this temperature.

The value for carbonic acid was calculated in the same way.

Regnault found that two thermometers filled respectively with air and carbonic acid ran almost parallel to about 300° ; and the same may be assumed, therefore, between 0° and 100° . Since, now, in the case of air, the values of k were known from what has been already said, k could also be calculated for carbonic acid at this temperature. The value at -87° C. was not calculated, as this is near the point of condensation.

The following values were found for k in the case of carbonic acid:—

Temp.	Pressure.		
	0·76.	1 metre.	2 metres.
3°	1·002050	1·000000	0·991322
50	1·003736	1·002509	0·997424
100	1·004488	1·003910	1·001157

In this case also the differences increase regularly with the pressure and with sinking temperature. The above values of k must here be again multiplied by the constant factor $K = 19\cdot0949$.

From this the three following formulæ of interpolation were calculated:—

$$\begin{aligned}
 0^{\text{m}}\cdot76 \quad . \quad . \quad k &= \left\{ 1\cdot006248 - \frac{0\cdot29401}{67\cdot03 + t} \right\} K, \\
 1^{\text{m}} \quad . \quad . \quad k &= \left\{ 1\cdot008229 - \frac{0\cdot88188}{104\cdot16 + t} \right\} K, \\
 2^{\text{m}} \quad . \quad . \quad k &= \left\{ 1\cdot014466 - \frac{3\cdot0382}{128\cdot27 + t} \right\} K.
 \end{aligned}$$

By differentiation there is obtained, at 0·76 metre, for instance,

$$\frac{dk}{dt} = \frac{0\cdot29401}{(67\cdot03 + t)^2} K,$$

from which,

$$\text{at } 3^{\circ}, \quad \frac{dk}{dt} = 0\cdot000059951 K;$$

$$\text{at } 100^{\circ}, \quad \frac{dk}{dt} = 0\cdot000010538 K.$$

In the case of nitrogen, the number of Regnault's observations is too small for calculating this formula. From the coefficients of expansion observed, it follows that k increases with the temperature; but in what manner k changes with the pressure at 100° cannot be determined, owing to a deficiency in the number of determinations of this coefficient under different pressures.

Now the formulæ found for air and carbonic acid show that the values of K for these gases must undergo a small correc-

tion. For air the number 29·2440, and for carbonic acid the number 19·0927 was found; but it was herein assumed that between 0° and 100° K changes regularly with the temperature. This is inadmissible; the new formulæ give

For air K=29·4443 $\log = 1·466041$,

For carbonic acid . K=19·0949 $\log = 1·280917$,

which values form the basis of all the calculations in this paper.

§ IV. On the Connexion between Atomic Weights and Volumes.

Gay-Lussac found, as is well known, that the volumes of gases which combine with one another stand to each other in a simple ratio; from which it follows that the same is the case between atomic weights and densities. Yet by comparing the densities determined by Regnault at 0° and 0·76 metre pressure, this relation is seen to be only approximately correct.

	Density.	Atomic weight.	Ratio of density.	Product.
H . . .	0·06926	1	1	0·06926
O . . .	1·10563	8	16	0·06910
N . . .	0·97137	14	14	0·06938
CO ² . . .	1·52901	22	22	0·06950

The differences in the values in the last column are manifestly too great to be ascribed to errors of observation, and they are therefore considered as arising from the deviations in Mariotte and Gay-Lussac's law. For carbonic acid, Regnault found the deviation at 0° and moderate pressure much smaller; and at 100° it totally disappeared. Yet this last result only proceeds from an error in calculation*.

The formulæ of interpolation which have been found render possible a more accurate examination. They give the changes produced by temperature and pressure in the value of k for the formula

$$pv = k\tau.$$

Since for this case the gases are always compared at the same pressure and temperature, p and τ are always equal, and v is proportional to k . It is sufficient, therefore, to compare the values of k for the different gases at the same pressure and temperature. This, however, was only possible for the gases H, N, and CO².

At 4° and 0·76 metre pressure, k is found to be for

H	422·337
N	$30·1094 \times 14 = 421·531$
CO ²	$19·1340 \times 22 = 420·948$

* Poggendorff's *Annalen*, vol. cxvi. p. 444.

At 4° and 0·5 metre pressure,

H	422·257
N	. . . 30·1220 × 14 =	421·708
CO ²	. . . 19·1761 × 22 =	421·874

At 100° and 0·76 metre pressure,

H	422·337
N	. . . 30·1386 × 14 =	421·940
CO ²	. . . 19·1806 × 22 =	421·972

At 100° and smaller pressure the calculation could only be made for carbonic acid; as in the case of nitrogen, the value of k at 100° is only known for 0·76 metre pressure.

At 0·5 metre we get

N	422·257
CO ²	. . . 19·1918 × 22 =	422·221

I believe that from this the conclusion may with certainty be drawn, that Gay-Lussac's law of volumes is quite valid; and that, therefore, from the atomic weights the ratio of the densities for the limiting condition of the gases may be accurately calculated. It is true that this in general has not been disputed; but when a physical principle is to be considered quite accurate, and therefore applicable as the starting-point of a theory, a new confirmation is never superfluous.

It must here be expressly remarked, that the formulæ obtained give the values of k accurately only within the data taken for the calculation. If the above values are calculated for a very small pressure, for instance 0·2 metre, or for 200°, greater differences are obtained, and sometimes even in opposite directions. For air, the values of k at 200° and different pressures were found almost exactly equal to each other; the formulæ of the preceding paragraph give then far greater differences than at 100°. Yet this does not disagree either with the theory or with the formulæ obtained. These do not in general give the real law of the changes in k ; the true formula is undoubtedly far more complicated. Yet, within the limits of pressure and temperature for which they are calculated, the empirical formulæ are sufficiently accurate; that is, the deviations which occur are not greater than the errors of observation. Within these limits they may safely be used in testing a theory.

If it were desired to calculate the formula with greater accuracy and in greater generality for these gases, they might be subjected to the condition of giving the theoretical density for pressure = 0, and very high temperature. Yet this calculation would be almost useless, and could not give much greater accuracy;

not for the reason that Regnault's determinations were not sufficiently accurate, but because the calculation of these experiments leaves much to be desired. Regnault, as is well known, deduced his formulæ from individual observations, instead of connecting them by means of the calculus of probabilities with all trustworthy observations.

§ V. Calculation of the Mechanical Equivalent of Heat.

This value was calculated from the well-known general formula*

$$c_1 - c = \frac{\tau}{J \left(\frac{d\tau}{dp} \right) \left(\frac{dv}{d\tau} \right)}.$$

Now the formula $p = k\tau$ gives

$$v dp = k d\tau + \tau \frac{dk}{dp} dp + \tau \frac{dk}{d\tau} d\tau,$$

from which is obtained

$$\frac{d\tau}{dp} = \frac{v - \tau \frac{dk}{dp}}{k + \tau \frac{dk}{d\tau}},$$

and, further,

$$p dv = k d\tau + \tau dk;$$

hence

$$\frac{dv}{d\tau} = \frac{1}{p} \left(k + \tau \frac{dk}{d\tau} \right).$$

By substituting both values, the formula becomes

$$c_1 - c = \frac{\left(k + \tau \frac{dk}{d\tau} \right)^2}{J \left(k - p \frac{dk}{dp} \right)}.$$

J is here to be calculated, since all the rest is known.

1. In my former paper the velocity of sound was taken at 332.77 metres, from which we obtain $\frac{c_1}{c} = 1.4128$. But this value must undergo a slight correction, since air contains aqueous vapour, and in the investigation it has been assumed that the values of $\frac{c_1}{c}$ are the same for air and for aqueous vapour;

* Zeuner, *Grundzüge der Wärmetheorie*, p. 177. W. Thomson *Phil. Mag.* S. 4. vol. iv. p. 168.

this, however, is not the case. Masson found the velocity of sound in hydrogen to be 401 metres against 333 in air; and from the formula for the velocity of sound, taking the density of aqueous vapour at $\frac{5}{8}$, and designating by γ' and γ the values of $\frac{c_1}{c}$ in the case of aqueous vapour and air, we get

$$401^2 : 333^2 = \frac{8}{5} \gamma' : \gamma,$$

from which

$$\gamma' = 0.9063\gamma.$$

The quantity of moisture present in the air during the experiments was $\frac{9}{744} \times \frac{5}{8} = \frac{1}{148}$ of the air.

Hence

$$148\gamma + 0.9063\gamma = 1.4128 \times 149,$$

$$\gamma = 1.4137 \pm 0.0008$$

for dry air at 0° and 0.76 metre pressure.

Taking the value of the velocity of sound as certain to $\frac{1}{1000}$, the corresponding limits of $\gamma = 1.4113$ and 1.4161 .

It is clear, from what follows, that this value of γ , which really is calculated for 0° , may without error be regarded as valid for 4° , for which temperature the above formula is calculated.

2. Regnault found for the specific heat of air

between -30° and $+10^\circ$	0.23771
0 „ 100	0.23741
0 „ 200	0.23751

In the calculation, c_1 was taken $= 0.2375$, from which is obtained

$$c_1 - c = 0.06947.$$

3. From the formula found in § III. we have

$$\frac{dk}{dt} = \frac{dk}{d\tau} = 0.0000060526 \text{ K.}$$

4. Further, $\tau = 27.15$, $p = 0.76 \times 13595.93 =$ the pressure in kilogrammes on the square metre.

5. From the formula for air at 4° , we get

$$k = 1.0002995 \times 29.2443;$$

and further, since $p = 13595h$ ($h =$ the barometric height),

$$\frac{dk}{dp} = \frac{dk}{13595dh} = -0.00000269 \text{ K.}$$

By substituting these values, the formula gives

$$J = 422 \cdot 10 \pm 0 \cdot 57 ;$$

if the probable error of the velocity of sound alone is considered,

$$\log J = 2 \cdot 62542.$$

Considering this velocity, again, correct to $\frac{1}{1000}$, the value of J must be between $423 \cdot 82$ and $420 \cdot 38$. All the other magnitudes used in the calculation could not possibly produce a greater error: the greatest error might still arise from the value of c_1 , although this passes in a lessened form into the value of $c_1 - c$.

The calculation has been given completely, in order the more readily to calculate the corrections which J would experience from a better determination of the elements. The statement of the limits within which the value of J may be taken as being tolerably certain was made in order, in the first place, to estimate the degree of trustworthiness which this determination can acquire, and, in the second place, because it leads to far more precise results if other determinations, such as those of Clausius and of Joule, undergo the same treatment. It is remarkable that the determinations, of Joule $= 423 \cdot 55$, of Clausius $= 421$, and of Bosscha, developed from galvanic measurements, $= 421 \cdot 1$ (Pogg. Ann. vol. cviii. p. 169), all fall within these limits; and the value which has been found is almost exactly the middle of these. Hence in the subsequent calculation I have adhered to this value, and not combined it with other results of whose degree of trustworthiness I could not judge so well.

[To be continued.]

XVIII. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, Ph.D., F.C.S.

[Continued from vol. xxx. p. 451.]

THE *Zeitschrift für Chemie* for May, 1865, contains the following abstract of a paper by Mendelejeff, in the Russian language, on the compounds of alcohol with water.

The first chapter the author devotes to a detailed and thorough criticism of all previous researches on this subject. He determines the magnitude of the errors of observation of the different authors, reduces all their weighings, and endeavours to make all determinations comparable by reducing them to the same unit. The author comes to the conclusion that the old observations of Gilpin (1794) are among the most accurate. With these the observations of Fownes (1847) and of Drinkwater (1848) closely agree. The numbers of Gay-Lussac and of Tralles cannot stand

a more accurate criticism ; while the latest observations of Baumhauer (1860) are charged with grave errors, and are far less valuable than the older researches.

The apparatus used by the author was constructed by Geissler of Bonn, and consisted of a cylindrical vessel of 20–30 millims. diameter. Into the upper end a thermometer divided into fifths and tenths of a degree, and on each side a capillary tube, somewhat inclined and accurately calibrated, were fused. One of these was widened at the end in an egg-shape ; and both could be closed by ground glass stoppers. The capacity of the apparatus was about 20–40 cubic centims. When one of the capillary tubes was connected by means of a bent tube with a flask containing alcohol, the apparatus could be filled with alcohol without bringing it into contact with air, by inhaling at the other capillary tube. The apparatus thus filled was placed in a water-bath, the temperature of which did not vary more than $\pm 0^{\circ} \cdot 02$ in half an hour. When the thermometer in the alcohol of the apparatus indicated the same temperature as the water-bath, the position of the liquid in the capillary tubes was read off by a cathetometer, and the apparatus weighed. Thermometers, balance, and weights were all accurately controlled ; and all observations were thus corrected for expansion of glass, irregularity of thermometer, difference of volume, magnitude of meniscus, errors in the weights and in the balance. The weighings are reduced to vacuum. All numerical statements refer to water at 4° as unity.

The author has taken into account all possible sources of error, and determined their magnitude. His determinations mostly agree to the fifth decimal place. The greatest deviations do not exceed 0·00008. Four determinations of the specific gravity of alcohol, made with different apparatus at different times, gave 0·000018 as the greatest difference. That is the limit of what can be attained with our present means, and the experiments of the author transcend in accuracy all previous ones.

The author bestowed special attention on the preparation of an absolutely pure alcohol. The spirit was prepared from corn-brandy, and contained 71·6 per cent. alcohol. 15 Eimer (of 12·7 litres) were first distilled over caustic lime and soda, then over freshly ignited charcoal, and finally repeatedly over fresh portions of caustic lime, the direct access of air being avoided as much as possible.

The author confirms the observation of Sömmering, that in the distillation of absolute alcohol, aqueous alcohol passes over first. Hence the specific gravity of those portions which first distil over is greater than that of the later portions ; hence also arises the too high specific gravity given for absolute alcohol.

Two other sources of error increase the specific gravity of alcohol,—first, the extraordinary tendency to absorb moisture from the air (an alcohol of 0.78973 had, after standing half an hour in an open flask, a specific gravity of 0.78992); and secondly, its property of absorbing air to a considerable extent. Alcohol of 0.78973 had, when shaken for five minutes in a closed flask, air being excluded, a specific gravity of 0.79007. It is known that alcohol saturated with air gives out air-bubbles when diluted with water. Pouillet and Baumhauer determined the specific gravity by hydrostatical processes, and their observations are thus charged with great errors.

The author attempted, but in vain, to keep absolute alcohol for a long time without alteration of weight: the best way was to keep it over lime under a bell-jar. For each observation a fresh portion of alcohol was rectified as rapidly as possible, access of air being prevented. This was effected by means of a specially constructed apparatus; and the alcohol was in all cases tested by the addition of water; if globules of air were formed, the specimen was rejected. In the distillation, the temperature of the water-bath should not exceed 83° to 85° ; if it do, the distillate will certainly contain water, in spite of all the lime.

The author has tested all the modes of dehydrating alcohol, and communicates his observations.

Calcined potash is well adapted for the dehydration of weak alcohol; but it does not remove the last traces, and at most an alcohol of 99 per cent. is obtained. *Chloride of calcium* and anhydrous *sulphate of copper* act in the same manner.

Caustic baryta, as proposed by Berthelot and Pean de St. Gilles, is admirably adapted for preparing absolute alcohol. When the supernatant liquid appears *yellow*, the alcohol has lost the last trace of water. For dehydrating a litre of alcohol, about 200 grms. baryta are needed. The author found it advantageous not to pour the dehydrated alcohol off, but, after standing for about a day, to distil it directly. When the quantity of baryta and alcohol was left to itself for a week, the liquid was orange-red in colour, and on distillation furnished an alcohol of higher specific gravity.

Dehydration by *sodium* the author totally rejected. If sodium-amalgam was used in order to moderate the action, some mercury passed over; and in all cases the distillate contained sodium: hence the determinations were too high.

The most practical method is that of caustic lime. The pieces must stand out of the alcohol, which must have at least a specific gravity of 0.792 at 20° . In two days all water is removed. But if it is desired to distil after the lapse of two or three hours, a previous heating to 50° or 60° is necessary. The

author usually added a little caustic baryta, and by the yellow colour the dehydration was recognized. The distillation was continued with the above precautions. The first portions exhibited too high a specific gravity; the means were concordant with each other, while the last portions had again too high a specific gravity. This latter fact is due to the circumstance that at a certain temperature *absolute alcohol takes water from hydrate of lime*.

From the numerous and concordant experiments of the author, the specific gravity of absolute alcohol (water at $4^{\circ}=1$) is

$$0\cdot78945 \text{ at } 20^{\circ}.$$

The observations which give numbers nearest to this are those of Muncke, $0\cdot7895$; Fownes, $0\cdot78959$; and Drinkwater, $0\cdot78958$; while Baumhauer found $0\cdot7899$, and Kopp even $0\cdot79277$.

By all appropriate tests the author convinced himself of the complete purity of the alcohol.

He found the boiling-point of absolute alcohol to be, as the mean of three very closely agreeing numbers, $78^{\circ}303$ (error $0^{\circ}01$). From the tension of alcohol-vapour Regnault determined it at $78^{\circ}28$.

To determine the coefficient of expansion, the specific gravity at different temperatures was determined. Experiment led to the following formula,

$$d = 0\cdot80625 - 0\cdot000834 \times t^{\circ} - 0\cdot00000029t^2,$$

from which the following values are obtained:—

Specific Gravity of Absolute Alcohol.

At 0°	$0\cdot80625$	At 20°	$0\cdot78445$
„ 5	$0\cdot80207$	„ 25	$0\cdot78322$
„ 10	$0\cdot79788$	„ 30	$0\cdot78096$
„ 15	$0\cdot79367$		

If, therefore, the volume of absolute alcohol at $0^{\circ}=1\cdot0$, it is at

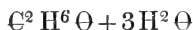
	Gay-Lussac.	Muncke.	Kopp.	Mendelejeff.	Baumhauer.
10°	$1\cdot01044$	$1\cdot01052$	$1\cdot01049$	$1\cdot0103$
15	$1\cdot01472$	$1\cdot01586$	$1\cdot01585$	$1\cdot01585$	$1\cdot0156$
20	$1\cdot02138$	$1\cdot02128$	$1\cdot02128$	$1\cdot0210$
30	$1\cdot03094$	$1\cdot03271$	$1\cdot03242$	$1\cdot03238$	$1\cdot0321$

The author's numbers agree best with those of Kopp, although the alcohol which Kopp used contained water. This agreement is owing to the fact that, as Mendelejeff found, a small addition

of water does not materially alter the coefficient of the expansion of alcohol.

In the fourth chapter, the author endeavours to determine the *contraction* which ensues when alcohol and water are mixed. For this purpose the specific gravity was taken of accurately weighed quantities of absolute alcohol and water free from air, at temperatures of from 0° to 30° . The different mixtures contain 40 to 50 per cent. of absolute alcohol. The observations were interpolated by means of the method of least squares. From these it follows that the greatest contraction takes place with a mixture which contains 45.88 per cent. of absolute alcohol. Such a mixture can be expressed very accurately by the formula $C^2 H^6 O + 3 H^2 O$, which requires 46 per cent. This mixture exhibits the greatest contraction at all temperatures.

The author sees in this a direct proof that the mixture



represents a more intimate combination of the alcohol with the water. From his observations on the maximum contraction, the following Table is taken. The contraction refers to 100 volumes of the resultant mixture of alcohol and water.

Percentage of absolute alcohol.	Contraction				
	at 0° .	10° .	15° .	20° .	30° .
40	4.0666	3.8180	3.7075	3.6060	3.4306
45	4.1440	3.8936	3.7821	3.6796	3.5023
46	4.1461	3.8956	3.7840	3.6815	3.5041
47	4.1441	3.8937	3.7823	3.6799	3.5027
50	4.1145	3.8678	3.7581	3.6575	3.4839

The author devotes the last chapter to the changes of specific gravity on mixing alcohol with water; and at the same time he tried to find an empirical formula for the dependence of the specific gravity on the composition of the mixture. The mixtures prepared by the author contained 40 to 100 per cent. of absolute alcohol. For weaker alcohol only a few observations were made; and for mixtures under 25 per cent., the observations of Gilpin and Drinkwater were used. The observations were all interpolated by the method of least squares. We give the following Table for the specific gravity of aqueous alcohol:—

Percentage of alcohol by weight.	Specific gravity of aqueous alcohol *			
	at 0°.	at 10°.	at 20°.	at 30°.
0	99988	99975	99831	99579
5	99135	99113	98945	98680
10	98493	98409	98195	97892
15	97995	97816	97527	97142
20	97566	97263	96877	96413
25	95115	96672	96185	95628
30	96540	95998	95403	94751
35	95784	95174	94514	93813
40	94939	94255	93511	92787
45	93977	93254	92493	91710
50	92940	92182	91400	90577
55	91848	91074	90275	89456
60	90742	89944	89129	88304
65	89595	88790	87961	87125
70	88420	87613	86781	85925
75	87245	86427	85580	84719
80	86035	8515	84366	83483
85	84789	83967	83115	82232
90	83482	82665	81801	80918
95	82119	81291	80433	79553
100	80625	79788	78945	78096

Caron† has discovered the existence of niobium and tantalum in a tin ore found at Montebbras, which is now being there worked as a source of tin. The quantity amounts to about 2 or 3 per cent., though in some cases as much as 5 per cent. has been extracted. Of two methods which he describes of extracting the niobium, the following is recommended as the best, though not the most expeditious.

The mineral, deprived of gangue and levigated, is fused with a mixture of charcoal and carbonate of soda, by which means a button of tin is obtained, the niobium being contained in the slag. This slag is then treated with a small quantity of hydrochloric acid, which removes the soda and a considerable portion of unreduced tin. The residue, consisting of silica, oxides of tin, niobium, manganese, iron, &c., is dissolved in a mixture of fluoric and sulphuric acids, diluted with water, filtered, and evaporated so as to expel the major part of the fluosilicic acid; it is then mixed with a large quantity of water and boiled, by which all the niobic acid of the liquid is precipitated. This precipitate of niobic acid is far from being pure; it contains much tin, a little iron, manganese, and perhaps tungsten. A prolonged digestion with hydrosulphate of ammonia removes the tin and the tung-

* Water at 4°=100000.

† *Comptes Rendus*, December 11, 1865.

sten, and the iron and manganese are then got rid of by dilute hydrochloric acid.

The author also succeeded in proving the existence in this mineral of tantalum, by means of Marignac's test with fluoride of potassium; but he was unable to obtain tantalic acid quite free from niobic acid.

The following *résumé* of a memoir on a new mode of testing mineral oils is given by the authors, MM. Salleron and Urbain*.

We have sought a method of testing the mineral oils employed in illumination which should be more exact than the determination of their density, or the direct measure of their inflammability. We show the inconveniences of each of these methods, and propose to substitute for them the measurement of the tension of the vapour of these liquids, which is proportional to their volatility, and therefore to their inflammability. We show that this method is more delicate than former ones. We then give the description of a new apparatus by which these tensions may be determined, and finally a Table containing the elastic forces of the vapour of one and the same oil taken as type; so that knowing, on the one hand, the tension of the oil to be tested corresponding to a given temperature, and, on the other, the tension at this temperature of the typical oil, by comparing these numbers the value of the specimen examined can be deduced. From our experiments we conclude that a tension of 64 millims. of water might be adopted as the limit of that which oils furnished to the public should possess.

Hoffmann† describes the following method of readily obtaining a pretty strong solution of peroxide of hydrogen. Potassium is melted in a porcelain crucible, and a current of air is blown on it by means of a bellows; in this way a yellowish-green mass is obtained which is rich in KO^3 . This is then added in small portions to tolerably concentrated fluosilicic acid, by which a certain quantity of oxygen is disengaged, at the same time that silicofluoride of potassium precipitates. The liquid, which is advantageously left acid by a small excess of silicofluoric acid, is considerably richer in HO^2 than that obtained by a slow oxidation of the metal. Tested with solution of permanganate of potash, the proportion of HO^2 in the liquid amounted to 1 in 735. When the silicofluoric acid was kept quite cool by means of a freezing-mixture, this proportion was doubled, and even more; in one case it amounted to 1 in 357.

By adding KO^3 to a solution of tartaric acid, a comparatively strong solution of HO^2 may also be obtained.

* *Comptes Rendus*, January 2, 1866.

† *Liebig's Annalen*, November 1865.

When ordinary phosphorus (that is to say, colourless and transparent phosphorus) is kept in water and is not exposed to a bright light, it becomes covered with a white opake crust, seldom very thick, which is known as white phosphorus.

According to Baudrimont, who has subjected this to examination*, this is neither a hydrate, nor an allotropic condition of normal phosphorus, nor does it arise from a devitrification. It is only ordinary phosphorus irregularly corroded on the surface and roughened, so to say, by the combustible action of air dissolved in water—a slow combustion which diffused light accelerates, and which ceases as soon as the liquid contains no more oxygen dissolved.

XIX. Notices respecting New Books.

A Treatise on Attractions, Laplace's Functions, and the Figure of the Earth. By JOHN H. PRATT, M.A., Archdeacon of Calcutta. Third Edition. Cambridge and London: Macmillan and Co. 1865.

AS this is the third edition of a well-known work, we need not do more than mention generally the nature of its contents, and then advert more particularly to one or two points in which it differs from foregoing editions. The first part, which is preliminary to the rest of the volume, contains in the first place the usual investigation of the attraction exerted by spheres, spheroids, and ellipsoids on internal and external points; next, an investigation of the properties of Laplace's functions, and their application to determining the attraction exerted by bodies nearly spherical on external and internal points; and lastly, an elaborate and interesting discussion of the attractions exerted by tablelands, mountains, &c. This last portion of the first part is illustrated by a series of examples of cases actually occurring in geodetic operations, particularly those of the Indian survey. It may be remarked that the attempt to take account of the effects of local attractions is the distinguishing feature of the book.

The second, and by far the larger part of the book is devoted to the question of the figure of the earth. This is discussed from two points of view,—first, theoretically, on the hypothesis of the earth's being, or having once been, a fluid mass; secondly, practically, as determinable by geodetic operations. Each of these discussions is very elaborate; and in the course of them there are passed in review a number of detached observations and investigations scattered through various scientific journals. Those who are interested in learning what has been done on this subject since the publication of Mr. Airy's celebrated Treatise, will obtain the requisite information from the present work, which is brief, clear, full, and worthy of Archdeacon Pratt's high reputation as a mathematician.

* *Comptes Rendus*, November 13, 1865.

We will now proceed to offer some observations on each of these discussions, and will begin with the second of them. It is scarcely necessary to mention that the mean surface of the earth is nearly or exactly that of an oblate spheroid, whose minor axis coincides with that of the earth's revolution; that its equatorial radius cannot differ much from 20,925,000 feet; and that its ellipticity, or the ratio which the excess of the equatorial radius over the polar radius bears to the former is about 1 : 300. If it be asked what is meant by the mean figure of the earth, and how it is determined, we may answer that the problem proposed for solution is really this,—Given a certain number of measured arcs of different meridians, to determine the geometrical figure of the surface on which they will most exactly fit. That figure when determined is the mean figure of the earth. There are three principal measured arcs which supply data for this determination: viz. the Russian arc, the latitudes of the extremities of which differ by about 25° ; the Anglo-French arc, in which that difference is about 22° ; and the Indian arc, in which the difference is about 21° . There are also other shorter arcs, *e.g.* those measured in Prussia, Peru, the Cape of Good Hope, &c. General Schubert, and subsequently Captain A. R. Clarke, have shown that the figure which most nearly represents these measurements is an ellipsoid. The calculation of the latter gentleman is based on the data supplied by thirteen stations on the Russian arc, twelve on the Anglo-French arc, eight on the Indian arc, five on the Cape arc, and two on the Peruvian arc. And his result comes to this, that the polar semiaxis is 20,853,768 feet, while the semi-major and minor axes of the equatorial ellipse are respectively 20,926,485 feet and 20,921,177 feet, the two latter numbers differing by just a mile. General Schubert makes the difference about half a mile. Archdeacon Pratt is of opinion that the method followed by these gentlemen is erroneous, inasmuch as it neglects the possible influence of local attraction on the plumbline at the standard stations of the arcs employed. The amount of the uncertainty introduced by this possibility is the subject of a very elaborate paper by Archdeacon Pratt, published in vol. xiii. of the Proceedings of the Royal Society. An account of the method pursued in that paper, and of the chief results arrived at, is given in the present work. By a preliminary investigation he shows that the differences between the latitudes of the principal stations on a measured arc, as determined by geodesy, are sensibly unaffected by local attraction; and consequently that if the latitude of that station to which the others are referred were clear of error depending on local attraction, the method pursued by Captain A. R. Clarke (Bessel's method) would be unobjectionable. But this is by no means the case. Accordingly Archdeacon Pratt investigates the consequences which follow from assuming that there exists a small unknown deflection of the plumbline at the reference-station, producing a small unknown error in the determination of its latitude. He then applies this investigation to determine the semi-major and minor axes, and the ellipticity of the mean ellipses which respectively represent the

Anglo-Gallic, Russian, and Indian arcs, involving in each case the small unknown correction (t) of the latitude of the primary station. Thus the Anglo-Gallic arc gives—

$$\text{Semi-major axis (in feet)} = 20928190 + 1577 \cdot 7t.$$

$$\text{Semi-minor axis} \quad \quad \quad = 20847200 - 5885 \cdot 9t.$$

$$\text{Ellipticity} \dots \dots \dots = \frac{1}{258 \cdot 4} (1 + 0 \cdot 0921 t),$$

where t is in seconds, and is to be reckoned positive if the deflection be northward. It appears, finally, that if the deflections are taken as $1'' \cdot 37$ for the Anglo-Gallic arc, $2'' \cdot 22$ for the Russian, and $0'' \cdot 033$ for the Indian arc, in all cases southerly, values are obtained for the semi-major and minor axes which in no case differ from their mean by so much as 300 ft. Accordingly the Archdeacon's final result is, that the mean figure of the earth is an oblate spheroid, and that its semi-major and minor axes are 20,926,189 feet and 20,855,316 feet respectively, and consequently the ellipticity is $\frac{1}{295 \cdot 3}$. Now, inas-

much as the possible existence of deflections of the assumed amount cannot be denied, the investigation shows conclusively that the principal measured meridian arcs supply data which by no means render it necessary, in the existing state of our knowledge, to suppose that the earth's mean figure is more correctly represented by an ellipsoid than by an oblate spheroid.

In regard to the second point. What may be compendiously termed the fluid hypothesis is this:—The earth took its form when in a fluid or semifluid state, under the action of the mutual attraction of its parts combined with their rotation round a common axis. As each point of such a mass must comply with the conditions of fluid equilibrium, the consequence of this hypothesis will be, that, at all events so long as the fluid state exists, the matter composing the earth is arranged in a succession of spheroidal layers having a common centre, the minor axes coinciding with the axis of rotation, their ellipticities decreasing gradually from the surface to the centre; the density of each layer will be the same throughout, but the densities of consecutive strata will gradually increase from the surface towards the centre. It is then assumed that in this state the earth gradually became solid from the surface inwards, a crust (possibly of great thickness) being thus formed over the fluid matter, but without disturbing to any considerable extent the arrangement above explained.

Independently of considerations drawn from any hypothesis as to the origin of the solar system, or from the facts of geology, it is held that there are good grounds for believing the fluid hypothesis to be true in fact. Thus Laplace says, "*La fluidité primitive des planètes est clairement indiquée par l'aplatissement de leur figure, conforme aux lois de l'attraction mutuelle de leurs molécules; elle est de plus prouvée pour la terre, par la diminution régulière de la pesanteur, en allant de l'équateur aux pôles*" (*Système du Monde*, vol. ii. p. 562, cd. 6). Mr. Airy says that "the results of pendulum observations,

lunar inequalities, and the precessional phenomena make it highly probable that the earth has once been in a fluid or semifluid state" (Conclusion to Treatise on the Figure of the Earth). And Archdeacon Pratt (p. 160) says, "we can hardly conceive stronger evidence than the foregoing pages furnish that the earth was once a fluid mass."

As the Archdeacon has brought forward an additional argument on the subject, and one of great importance if sound, we will briefly pass the whole in review, as stated in the present work.

In the first place, the variations of the force of gravity on the earth's surface deduced from this hypothesis exactly coincide with those which are ascertained by means of experiments with pendulums. In the next place, the moon's motion in latitude experiences a perturbation which falls in exactly with the hypothesis. In the third place, by making a particular assumption as to the variation of the densities of the consecutive strata, a value of the ellipticity of the mean surface is deduced which nearly coincides with that ascertained by geodesy. In the fourth place, on the same assumption, an expression for the annual precession can be deduced which, when compared with that which actually exists, gives an ellipticity for the mean surface not differing much from that ascertained by geodesy.

Such being the evidence, let us now consider its value. It is plainly all of one kind. An hypothesis is framed, and is found to lead to results coinciding with observed facts. This may be presumptive evidence of the truth of the hypothesis, but is not proof. If a large number of phenomena of various kinds are connected and explained by an hypothesis, the presumption is strong, and may amount to something equivalent to proof. But if the facts are few, the presumption cannot be strong; and if to enable it to embrace these few facts it has to be backed by a subsidiary hypothesis, the presumption in its favour is very much weakened. And this seems to be the case with the fluid hypothesis. The third and fourth "tests" depend on a particular assumption as to the variation of the densities of the successive layers, viz. that expressed by the formula

$$\rho a = Q \sin qa,$$

where ρ denotes the density of the layer whose semi-major axis is a , Q and q being constants. Now this formula does incidentally imply that the density increases as the centre is approached, and is so far unobjectionable; but its chief recommendation is that it possesses the unique property of rendering a certain differential equation integrable. It would, to our mind, be a singularly happy coincidence if a law of density assumed for such a purpose were physically true. We think, therefore, that the real evidence for the fluid hypothesis is reduced to that furnished by the first two "tests."

Now, with regard to them, it is plain that any hypothetical arrangement of the matter composing the earth would possess the same amount of consistency with these facts as the fluid hypothesis, provided that the matter when so arranged continues to exert the same attraction as before on *all* external points. Hence in order to

complete the proof of the fluid hypothesis, it is necessary to show that no such arrangement is possible. And this Archdeacon Pratt attempts to do. He says (p. 80), "Suppose some change were made in the arrangement of the earth's mass without altering its external form. It is evident that, although the resultant attraction of the whole mass on the surface might possibly be unaltered by this change at particular points of the surface, it could not remain the same as before at every point of the surface;" and then follows some general reasoning too long to quote. Not content with this, he calculates the separate effects of three hypothetical rearrangements, and finds them all inconsistent with the results of pendulum experiments. It must be allowed that the pains bestowed upon this point show that the Archdeacon is awake to the requirements of strict proof. But we must add that in our opinion he has failed to prove his point. It admits of easy demonstration, that *without changing the form of a given mass it is possible to rearrange its particles in an indefinitely great number of ways consistently with its attraction on all external points continuing the same as before*. We are not aware that this fact has ever before been pointed out; and as it may be disputed, we add a demonstration in a postscript.

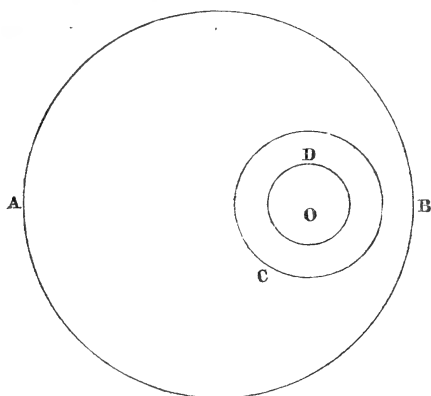
It is perhaps worth while to point out one consequence that follows from this theorem. It is stated that the variation of gravity on the earth's surface proves that the matter of the earth must be arranged according to the fluid hypothesis; so far is this from being true, that it need not be inconsistent with the ascertained variations of gravity if there were distributed irregularly through the earth very dense masses of matter, and likewise cavities filled with very light forms of matter.

Let us, however, not be mistaken; we are not advocating any rival theory. If we must have a theory, we incline to accept that according to which the earth was once in a fluid or semifluid state. But we do not see the need of having any theory at all. The fluid hypothesis undoubtedly can be made to comply with all the facts of the case; but we think it follows from what has been said above, that the evidence derived from the figure of the earth in favour of its actual truth does not rise above a slight probability, and that under any circumstances Archdeacon Pratt has very much over-estimated the force of that evidence.

P.S.—Conceive any mass attracting an external point (P), and conceive the mass to consist of two portions V and V'; these two portions may be distinct, or may be superimposed wholly or in part in any way whatever. Now the attraction of the whole mass on P must be the resultant of the separate attraction of V and V' on P. Conceive (if possible) the matter composing V' to undergo any change of arrangement and become v' , such that its attraction on P remains the same; then the attraction of $V+V'$ on P will be the same as that of $V+v'$ on P.

Suppose V' to be a spherical shell of uniform density; then if the matter composing it be rearranged so as still to form a concentric

spherical shell of uniform density (v'), there will be no change in the attraction which it exerts on any point external to both. Thus suppose AB to be a section of the earth made by the plane of any meridian, and suppose its matter to be solid and arranged in any



manner. Take any point O , and with centre O and any radii OC and OD describe spherical shells; suppose a uniformly distributed portion of the matter composing the former shell to be removed and to be uniformly distributed on the latter shell. This can be done without disturbing the attraction of the whole mass on any external point.

Since the point O may be taken anywhere within the earth, and since the radii OC and OD are subject to no condition except that the spheres whereof they are radii must fall wholly within AB , and since the density of the transferred matter may have any value not exceeding the least density of the part from which it is removed, it is clear that the number of particular cases included in the proof is indefinitely great.

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxx. p. 318.]

December 21, 1865.—Sir Henry Holland, Bart., Vice-President, in the Chair.

THE following communications were read:—

“On the Expansion of Water and Mercury.” By A. Matthiessen, F.R.S.

Before commencing a research into the expansion of the metals and their alloys, it was necessary to prove that the method I intended to employ, namely that of weighing the metal or alloy in water at different temperatures, would yield good and reliable results.

Phil. Mag. S. 4. Vol. 31. No. 207. Feb. 1866. M

To check, therefore, the method, I was led to determine the coefficient of expansion of mercury, and, basing my calculations on Kopp's coefficients of expansion of water, I expected to obtain Regnault's coefficient of expansion of mercury. The coefficient deduced from experiments did not agree with Regnault's; and being unable to discover any source of error in the method of experimenting, I determined to reinvestigate the matter.

The memoir is divided into four parts.

I. On the determination of the coefficients of the linear expansion of certain glass rods.

These rods (1825 millims. long and of 20 millims. diameter) were kindly made for these experiments by Mr. F. Osler. The method used for the determination of their increment in length was that of measuring it with a micrometer-screw, with which a length could be measured with accuracy to 0.001 millim.

The rod was placed in a long trough, the one end of the rod resting against a fixed glass tube capped with zinc, the other against another glass tube the other end of which rested against the micrometer-screw. Water was allowed to flow through these glass tubes during the time of observation. The trough being filled with water at ordinary temperature and the position of the screw read off, the water was heated to boiling and another reading taken.

The mean of sixteen observations gave for the linear expansion of these rods

$$L_t = L_0 (1 + 0.00000729t),$$

and therefore for the cubical expansion

$$V_t = V_0 (1 + 0.00002187t).$$

II. On the method employed for the determination of the cubical expansion of water and mercury.

This part of the paper contains a full description of the apparatus employed, and the precautions taken.

The method consists of weighing the substances in water at different temperatures, and from the loss of weight in water deducing its volume. For this deduction, the expansion of water at different temperatures is required.

III. On the redeterminations of the coefficients of expansion of water.

To determine these, pieces of the glass rods (the linear expansion of which had to be determined), ground to the shape of a double wedge, were weighed in water of different temperatures. Three pieces of glass were used (making three series), the weighings being made at temperatures between 0° and 100°, the whole number of observations being thirty-two.

From these it was found that the expansion of water between 4° and 100° may conveniently be expressed between 4° and 32° by the formula

$$V_t = 1 - 0.0000025300(t-4) + 0.0000083890(t-4)^2 \\ - 0.00000007173(t-4)^3,$$

and between 32° and 100° by

$$V_t = 0.999695 + 0.0000054724t^2 - 0.000000011260t^3.$$

The values calculated from these formulæ for the volume occupied by water at different temperatures are given in Table I. from degree to degree, together with the differences for each degree.

TABLE I.

T°. C.	Volume of water at T°.	Difference per 1°.	T°. C.	Volume of water at T°.	Difference per 1°.	T°. C.	Volume of water at T°.	Difference per 1°.
4	1.000000		37	1.006616	0.000355	69	1.022050	0.000598
5	1.000006	0.000006	38	1.006979	363	70	1.022648	604
6	1.000028	22	39	1.007351	372	71	1.023252	609
7	1.000066	38	40	1.007730	379	72	1.023861	616
8	1.000119	53	41	1.008118	388	73	1.024477	622
9	1.000188	69	42	1.008514	396	74	1.025099	628
10	1.000271	83	43	1.008918	404	75	1.025727	634
11	1.000369	98	44	1.009331	413	76	1.026361	639
12	1.000479	110	45	1.009751	420	77	1.027000	646
13	1.000604	125	46	1.010179	428	78	1.027646	650
14	1.000742	138	47	1.010614	435	79	1.028296	657
15	1.000892	150	48	1.011059	445	80	1.028953	662
16	1.001054	162	49	1.011510	451	81	1.029615	668
17	1.001227	173	50	1.011969	459	82	1.030283	673
18	1.001412	185	51	1.012435	466	83	1.030956	678
19	1.001608	196	52	1.012909	474	84	1.031634	684
20	1.001814	206	53	1.013391	482	85	1.032318	689
21	1.002029	215	54	1.013879	488	86	1.033007	694
22	1.002254	225	55	1.014376	497	87	1.033701	699
23	1.002488	234	56	1.014879	503	88	1.034400	704
24	1.002731	243	57	1.015390	511	89	1.035104	709
25	1.002982	251	58	1.015907	517	90	1.035813	714
26	1.003241	259	59	1.016432	525	91	1.036527	718
27	1.003507	266	60	1.016964	532	92	1.037245	724
28	1.003780	273	61	1.017502	538	93	1.037969	728
29	1.004059	279	62	1.018047	545	94	1.038697	732
30	1.004345	286	63	1.018599	552	95	1.039429	737
31	1.004635	290	64	1.019158	559	96	1.040166	741
32	1.004931	296	65	1.019724	566	97	1.040907	746
33	1.005249	318	66	1.020296	572	98	1.041653	751
34	1.005578	329	67	1.020874	578	99	1.042404	755
35	1.005916	338	68	1.021459	585	100	1.043159	
36	1.006261	0.000345			0.000591			

IV. On the redetermination of the coefficient of expansion of mercury.

The pure mercury was weighed in a bucket in the water at different temperatures. The glass bucket was made from the end of a test-tube (its length being about 20 millims. and width 15 millims.). The expansion of this sort of glass was found to be

$$V_t = V_0 (1 + 0.00002566t).$$

Five series were made with mercury ; and its expansions, deduced from the water-expansions given in Table I., were

$$\text{Series I.} \dots\dots V_t = V_0 (1 + 0.0001815t),$$

$$\text{Series II.} \dots\dots V_t = V_0 (1 + 0.0001813t),$$

$$\text{Series III.} \dots\dots V_t = V_0 (1 + 0.0001808t),$$

$$\text{Series IV.} \dots\dots V_t = V_0 (1 + 0.0001808t),$$

$$\text{Series V.} \dots\dots V_t = V_0 (1 + 0.0001816t),$$

$$\text{Mean} \dots\dots V_t = V_0 (1 + 0.0001812t),$$

a value closely agreeing with Regnault's, namely

$$V_t = V_0 (1 + 0.0001815t).$$

Calculating from the five series the coefficients of expansion of mercury, using Kopp's water-expansion (taking the volume at $4^\circ = 1$), we find as mean

$$V_t = V_0 (1 + 0.000178t).$$

In the following Table I give the values obtained by different observers for the volumes occupied by water at different temperatures, the volume at 4° being taken equal to 1.

TABLE II.

T.	Kopp*.	Despretz†.	Pierre‡.	Hagen§.	Matthiessen.
0	1.000000	1.000000	1.000000	1.000000	1.000000
4	1.000000	1.000000	1.000000	1.000000	1.000000
10	1.000247	1.000268	1.000271	1.000269	1.000271
15	1.000818	1.000875	1.000850	1.000849	1.000892
20	1.001690	1.001790	1.001717	1.001721	1.001814
30	1.004187	1.004330	1.004195	1.004250	1.004345
40	1.007654	1.007730	1.007636	1.007711	1.007730
50	1.011890	1.012050	1.011939	1.011994	1.011969
60	1.016715	1.016980	1.017243	1.017001	1.016964
70	1.022371	1.022550	1.023064	1.022675	1.022648
80	1.028707	1.028850	1.029486	1.028932	1.028953
90	1.035524	1.035660	1.036421	1.035715	1.035813
100	1.043114	1.043150	1.043777	1.042969	1.043159

Kopp, Despretz, and Pierre used the same method for their determinations—that of determining the expansion of water in glass vessels (dilatometers). Hagen employed the weighing process, but at high temperatures employed no special precautions to prevent the steam condensing on his fine wire ; hence his values at 90° and 100° fall below mine.

* Pogg. Ann. xcii. 42.

† Ann. de Chim. et de Phys. lxx. (1^{re} sér.) 1.

‡ Ann. de Chim. et de Phys. xiii. (3^{me} sér.) 325. Calculated by Frankenheim, Pogg. Ann. xvi. 451.

§ Abhandlungen d. k. Acad. der Wissensch. zu Berlin, 1865.

It will be seen from the foregoing Table that Kopp's values are lower than the others; and bearing in mind that the coefficient of expansion of mercury, when deduced by means of these, falls below that obtained by Regnault, but when deduced from Despretz's or my own agrees closely with Regnault's, we are led to conclude that Kopp's values must be somewhat incorrect.

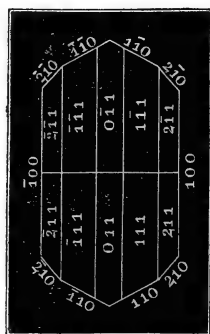
"On the Forms of some Compounds of Thallium." By W. H. Miller, M.A., For. Sec. R.S., Professor of Mineralogy in the University of Cambridge.

Nitrate of Thallium.

Prismatic, $010, 011 = 38^\circ 8'1$; $100, 110 = 62^\circ 56'3$.

100, 011	$90^\circ 0'$
100, 110	$62^\circ 56'3$
100, 210	$44^\circ 23'$
100, 111	$68^\circ 6'5$
100, 211	$51^\circ 13'$
110, 111	$34^\circ 57'5$
$011, 0\bar{1}1$	$103^\circ 44'$
$011, 211$	$38^\circ 47'$
$110, \bar{1}10$	$54^\circ 7'4$
$210, 211$	$28^\circ 46'$
$210, \bar{2}10$	$91^\circ 14'$
$011, 111$	$21^\circ 53'5$
$111, \bar{1}11$	$43^\circ 47'$
$111, 1\bar{1}1$	$93^\circ 44'8$
$111, \bar{1}\bar{1}1$	$110^\circ 5'$
$211, \bar{2}11$	$77^\circ 34'$
$211, 2\bar{1}1$	$75^\circ 38'$
$211, \bar{2}\bar{1}1$	$122^\circ 28'$

Fig. 1.



Observed combinations:— $100, 111$; $100, 111, 211$; $100, 011, 111, 211$; $100, 110, 210, 111, 211$; $100, 011, 110, 210, 111, 211$.

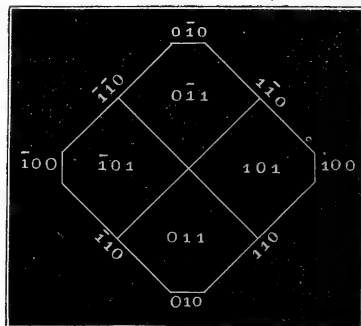
No cleavage observable.

From the observed minimum deviation of the brightest part of the solar spectrum formed by refraction through the faces $100, \bar{1}10$, it appears that the index of refraction of a ray in the plane 001 , and polarized in that plane, is about 1.817. The refrangibility of the other ray is greater, its minimum deviation through the same faces being 93° nearly.

*Sulphocyanide of Thallium.*Pyramidal, $001, 101 = 38^\circ 20' 3''$.Observed forms:— $100, 110, 101$.

Fig. 2.

$100, 010$	$90^\circ 0'$
$100, 110$	$45^\circ 0'$
$100, 011$	$90^\circ 0'$
$100, 101$	$51^\circ 39' 7''$
$110, 101$	$63^\circ 59'$
$101, \bar{1}01$	$76^\circ 40' 6''$
$101, 011$	$52^\circ 2'$

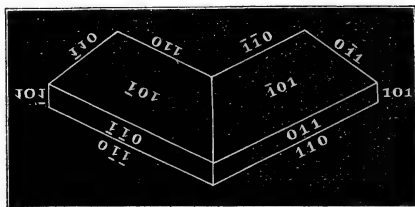
Observed combinations:— $110, 101; 100, 110, 101$.

The crystals are remarkable for the very unequal extension of the faces of the same simple form, and at first sight look as if they belonged to the oblique system. The breadth and thickness of one of the largest crystals were 1.1 and 0.055 millimetre respectively; and of two adjacent faces of the form 101 , one was about eleven times the breadth of the other. The distribution of the large and small faces did not appear to be subject to any law; so that these crystals cannot be regarded as combinations of large and small hemihedral forms.

Twins. Twin face 101 .

Fig. 3.

$101, \bar{1}01$	$180^\circ 0'$
$110, 011$	$52^\circ 4'$
$\bar{1}\bar{1}0, 011$	$-52^\circ 4'$
$011, 110$	$75^\circ 56'$
$011, \bar{1}\bar{1}0$	$75^\circ 56'$
$\bar{1}01, 101$	$26^\circ 38' 8''$



No cleavage observable.

An attempt was made to determine the optical constants of the crystal by observing the minimum deviation of light refracted through a face of the form 110 and one of the opposite faces of the form 100 ; the latter were, however, so small that the observation could not be made with much accuracy. It appeared that for the ordinary ray polarized in a plane parallel to the line 001 , the indices of refraction of red light, of the brightest part of the spectrum, and of violet light were about 2.15, 2.159, and 2.314 respectively, and that, for the extraordinary ray polarized in the plane 001 ,

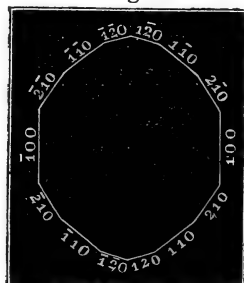
the indices of refraction of red light, the brightest part of the spectrum, and of violet light were about 1·890, 1·973, and 2·143 respectively.

Carbonate of Thallium.

The faces which have been observed are all in one zone, and exhibit a symmetry which is compatible with either the prismatic or the oblique system. The crystals probably belong to the prismatic system. They are aggregated in such a manner as to render it very difficult to isolate a single crystal, or to determine the faces which belong to the different individuals of a group of crystals.

Observed forms :—1 0 0, 1 1 0, 2 1 0, 1 2 0. Fig. 4.

1 0 0, 1 1 0	51° 28'
1 0 0, 2 1 0	32 7
1 0 0, 1 2 0	68 57
1 1 0, $\bar{1}$ 1 0	77 4



Twins. Twin face 1 1 0. One individual is generally united to each of two others, in this respect resembling the twins of cerussite, aragonite, glaserite, and chrysoberyl.

A cleavage has been observed probably parallel to the faces of the form 1 1 0; it may, however, be parallel to the faces of the form 1 0 0, the complexity of the twin crystals being such that it could not be ascertained whether the cleavages observed belonged to one crystal or to two different crystals.

I am indebted to Mr. Crookes, the discoverer of thallium, for the crystals of nitrate, sulphocyanide, and carbonate of thallium, above described.

GEOLOGICAL SOCIETY.

[Continued from vol. xxx. p. 453.]

November 22, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read :—

1. "On the impressions of Selenite in the Woolwich Beds and London Clay." By P. Martin Duncan, M.B., Sec. G.S.

Spaces formerly occupied by crystals of Selenite having been described by the author as occurring in Woolwich Beds near Mottingham, Kent, and in the unfossiliferous London Clay of Tending Hundred, he endeavoured to account for the phenomena to which he had drawn attention. The various facts bearing on the question, including the conditions under which the beds were depo-

sited, their chemical composition, and the mineral condition of the fossils, having been described in detail, Dr. Duncan proceeded to discuss the explanations that could be suggested to account for the formation and subsequent disappearance of the crystals. He came to the conclusion that the mineral had resulted from the action of sulphuric acid, contained in percolating water, on preexisting carbonate of lime, the sulphuric acid having been formed by the oxidation of sulphuretted hydrogen by the oxygen evolved from the decomposing vegetable remains occurring in the Plant-beds intercalated in the strata containing Selenite-spaces. The hydrocarbons resulting from the same decomposition would in solution be sufficient to produce the decomposition of the Selenite. In conclusion Dr. Duncan urged that, if his explanations were accepted, the occurrence of Selenite in a deposit must be held to prove the former existence of organisms in it, and the removal of the Selenite to be equivalent to the loss of the evidence of such existence; therefore there can be no reason why the purest clay-slate may not have been once as fossiliferous as the Woolwich Beds.

2. "On the Relation of the Chillesford Beds to the Norwich Crag." By the Rev. O. Fisher, M.A., F.G.S.

The geological position of the Chillesford Clay has never been definitely settled. Mr. Prestwich, who first described it, left the question open as to its identity with the Norwich Crag, or with the more recent marine, freshwater, and land series which immediately underlies the great northern Clay-drift of Norfolk. Sir C. Lyell supports the former view, while Mr. S. V. Wood, Jun., considers the Chillesford Clay a local member of his "Middle Drift." The author described the Chillesford Beds as they occur at Chillesford, and thence traced them northward to Aldborough. At Thorp, north of Aldborough, the Norwich Crag is exposed; and the main object of the paper was to show that this bed probably overlies the Chillesford Clay. In order to prove that this Crag is not identical with the *Mya*-bed below the clay, Mr. Fisher cited its greater thickness, its difference in lithological character, and the dissimilarity of their fossils; he also remarked that it rested upon a loamy clay, and contained a strong spring at its base, whereas the *Mya*-bed was always observed to rest on porous beds; he therefore inferred that this loamy clay was the Chillesford Clay, and showed that the gentle dip to the north would bring it into the required position; moreover he had found indurated nodules of loam, resembling weathered Chillesford loam, in the base of the Norwich Crag at this locality. Mr. Fisher next noticed the occurrence of the same beds at Southwold, and stated that the well-known deposit from which the late Colonel Alexander obtained so many mammalian remains was the *Mya*-bed. The Norwich Crag is also seen in this neighbourhood at Wangford, differing in character from the *Mya*-bed, and resting on a loamy clay resembling, and probably identical with, the Chillesford Clay. The sequence of these beds is therefore, in descending order:—(1) Norwich Crag; (2) Chillesford Clay; (3) *Mya*-bed; (4) Red Crag.

December 6, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Western Limit of the Rhætic Beds in South Wales, and on the position of the Sutton Stone." By E. B. Tawney, Esq., F.G.S. With a Note on the Corals of the Sutton Stone, by P. Martin Duncan, M.B., Sec. G.S.

Mr. Tawney commenced with a description of the Rhætic beds as they occur near Pyle station, west of Bridgend, and at Cwst y Coleman, north-west of that place, giving detailed sections of the beds at these localities, and showing the distribution of the fossils in them. The author then described the characters of the "Sutton Stone," and showed its relations to the beds above and below, giving to the building-stones generally called "Sutton Stone" the name "Sutton Series," and to the beds which intervene between the Sutton Stone and the base of the true Lias, and which have hitherto been considered Lias, the name "Southerndown Series," illustrating the stratigraphical features by a general section from Sutton to Dunraven Castle, and by vertical sections at Southerndown and Laleston. From the evidence yielded by the fossils, the author was of opinion that the Southerndown series belonged to the Rhætic formation, and must be separated from the Lias—that the Sutton series is somewhat older than the *Avicula-contorta* beds, and has affinities with the Trias—and that, by the discovery of *Ammonites* in the Sutton beds, the first appearance of that genus in the British area has been proved to have occurred during a period anterior to the Lias.

In his note on the Corals, Dr. Duncan stated that, besides two species derived from the Carboniferous Limestone, he had been able to determine four species of *Zoantharia* from the base of the "Sutton Stone." These Corals are unlike any hitherto discovered in North-western Europe, and, with certain reservations, were said to indicate an horizon which, in the Alpine Triassic districts, would be deemed St. Cassian; but, as our knowledge of the vertical range of the St. Cassian Corals is at present very imperfect, their absolute age cannot be more definitely stated, their occurrence in South Wales rendering it probable that they have a greater vertical, as they are now proved to have a greater horizontal, range than has hitherto been supposed.

2. "Notes on a Section of Lower Lias and Rhætic Beds near Wells, Somerset." By the Rev. P. B. Brodie, M.A., F.G.S.

A section recently exposed at Milton Lane, one mile and a half north of Wells, exhibited the *Lima*-beds passing into and overlying the White Lias and *Avicula-contorta* zone. The author described the section (which was constructed by Mr. J. Parker and himself) in detail, and showed that the *Lima*-series attained here a thickness of 10 feet 4 inches, and the Rhætic beds, including the grey marls, of 18 feet 6 inches; he was not able to discover any trace of *Ammonites planorbis*, or of any of the peculiar limestones indicating the "Insect" and "Saurian" zones. He found one fragment from a

bone-bed lying loose at the end of the lane, and containing characteristic fish-remains ; but though he searched carefully, he could not find *in situ* the bed from which it had been detached.

December 20, 1865.—Sir Charles Lyell, Bart., in the Chair.

The following communication was read :—

“On the Conditions of the Deposition of Coal, more especially as illustrated by the Coal-formation of Nova Scotia and New Brunswick.” By J. W. Dawson, LL.D., F.R.S., F.G.S.

In several former papers Dr. Dawson has endeavoured to illustrate the arrangement of the Carboniferous rocks of Nova Scotia, and to direct attention to their organic remains, the structures found in the coal, and the evidence which they afford as to the mode of accumulation of that mineral. In this paper the author summed up and completed his researches, adding some new facts resulting from the study of the microscopic structure of more than seventy beds of coal occurring in the South Joggins section, and of the fossil plants associated with them.

Some general considerations relating to the physical conditions of the Carboniferous period in Nova Scotia were first given, the author dividing the strata representing that period into (1) the Upper Coal formation, (2) the Middle Coal formation, (3) the “Millstone-grit” formation, (4) the Lower Carboniferous marine formation, and (5) the Lower Carboniferous Coal-measures, describing the characters of these divisions in detail, and giving a sketch of the physical conditions which prevailed during their deposition. He was of opinion that we must regard each of the above-mentioned divisions as the evidence of a period presenting during its whole continuance the diversified conditions of land and water, with their appropriate inhabitants, and as forming a geological cycle in which such conditions were to a certain extent successive.

As in previous publications, so in this, Dr. Dawson contended that the occurrence of *Stigmaria* under nearly every bed of coal proves beyond question that the material of the coal was accumulated by growth *in situ*, while the character of the intervening strata proves the abundant transport of mud and sand by water ; in other words, the conditions implied are such as prevail in the swampy deltas of great rivers. He also stated that the coal consists principally of the flattened bark of Sigillarioid and other trees, mixed with leaves of ferns, *Cordaites*, &c., and other herbaceous débris, and that the Cannel Coal and Earthy Bitumen are of the nature of the fine vegetable mud which accumulates in the ponds and shallow lakes of modern swamps.

In the succeeding portions of the paper the author gave details of the character and contents of the several beds of coal in the Joggins section, arranged in the order of Sir W. E. Logan’s sectional list, and made some remarks on the genera of animals and plants whose remains occur in the coal, and on their evidence as to the mode of its accumulation.

January 10, 1866.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Origin and Microscopic Structure of the so-called Eozoon-Serpentine." By Prof. W. King and Dr. T. H. Rowney.

Taking the Grenville Rock as its type, "Eozoonal Serpentine" was defined by the authors to consist essentially of variously formed granules of Chrysotile, or some other allied mineral, imbedded in, or intermixed with, Calcite. Although differing from the type in some respects, the varieties of Serpentine which they have examined from Connemara, Donegal, the Isle of Skye, India, Bavaria, and the State of Delaware are considered to belong to the same section. The Serpentine from Cornwall, the Isle of Anglesea, and Saxony, which appears to be devoid of "Eozoonal" structure, they were disposed to look upon, but with considerable doubt, as an eruptive rock. The authors stated their conviction that every one of the presumed organic structures of "Eozoonal" Serpentine is purely and primarily mineral or crystalline. The "skeleton" they held to be identical with the calcareous matrix of certain minerals, notably Chondrodite, Pargasite, &c. They adduced various considerations and evidence to show that the "proper wall" cannot have resulted from pseudopodial tubulation; and, instead of being an independent structure, in their opinion it is no more than the surface-portion of the granules of Chrysotile crystallized into an asbestiform layer. The dendritic and other forms, considered to represent the "canal system," were shown to be tufts of Metaxite, or some other allied variety of Chrysotile; while the resemblance they bear to some which are common in crystalline limestones, also their identity to the imbedded crystallizations of native silver, moss agates, &c., and the total dissimilarity between them and the foraminiferal structures with which they have been homologued, are points which the authors held to be conclusively fatal to the view which contends for such forms being of organic origin; in their opinion they are no more than imbedded "imitative" crystallizations. What have been taken for "Stolons," they were convinced are for the most part crystals of Pyrosclerite. The "chamber-casts" were considered to be identically represented among both minerals and rocks,—in the former by the grains of Chondrodite, Pyralloolite, Pargasite, &c.; and in the latter by the segmented kernels of native copper, Zeolites, &c., in eruptive rocks; also by the remarkable botryoidal and other shapes which occur in the Permian limestone of Durham. The authors concluded by offering it as their opinion that "Eozoonal" Serpentine is a metamorphic rock; and they threw out the suggestion that it may in many cases have also undergone a pseudomorphic change; that is, it may have been converted from a gneissoid calcareous Diorite by chemical introductions or eliminations.

2. "Supplemental Notes on the Structure and Affinities of *Eozoon Canadense*." By W. B. Carpenter, M.D., F.R.S., F.G.S.

In this paper Dr. Carpenter stated that a recent siliceous cast of *Amphistegina* from the Australian coast exhibited a perfect representation of the "asbestiform layer" which the author described in

his former communication on the structure of *Eozoon*, and which led him to infer the Nummuline affinities of that ancient Foraminifer—a determination which has since been confirmed by Dr. Dawson. This “asbestiform layer” was then shown to exhibit in *Eozoon* a series of remarkable variations, which can be closely paralleled by those which exist in the course of the tubuli in the shells of existing Nummuline Foraminifera, and to be associated with a structure exactly similar to the lacunar spaces intervening between the outside of the proper walls of the chambers and the intermediate skeleton by which they become overgrown, formerly inferred by the author to exist in *Calcarina*.

Dr. Carpenter then combated the opinion advanced by Professor King and Dr. Rowney in the preceding paper, and stated that, even if the remarkable dendritic passages hollowed out in the calcareous layers, and the arrangements of the minerals in the Eozoic limestone, could be accounted for by inorganic agencies, there still remains the Nummuline structure of the chamber-walls, to which, the author asserts, no parallel can be shown in any undoubted mineral product.

In conclusion the author stated that he had recently detected *Eozoon* in a specimen of Ophicalcite from Cesha Lipa in Bohemia, in a specimen of gneiss from near Moldau, and in a specimen of serpentinous limestone sent to Sir Charles Lyell by Dr. Gümbel of Bavaria.

XXI. Intelligence and Miscellaneous Articles.

ON THE MEASUREMENT OF SMALL FORCES BY MEANS OF THE PENDULUM. BY MM. JAMIN AND BRIOT.

WHEN a spherical ball, suspended by a flexible wire attached to a fixed point, is removed but a little from the vertical, and has imparted to it a very small initial velocity in any direction, its centre describes approximately an ellipse situated on a horizontal plane. When the amplitude of the oscillations is sufficiently small, the two axes of the ellipse may be regarded as constant in magnitude and in direction. If there is no initial velocity, the small axis of the ellipse is equal to zero, and the motion is in a plane.

But if on the pendulum in motion a force is made to act other than that of gravity, and relatively very small, this force produces in the elliptical motion of the pendulum modifications or perturbations which we propose to study.

We first treated the question analytically by aid of the method of the variation of constants devised by Lagrange. The constants or elements in this case are four; that is, half the major axis a of the ellipse, half the minor axis b , the angle α which the major axis makes with a fixed right line, and the time τ of the passage to the summit of the major axis. Denoting by R what may be called the perturbing function, the differential equations of the disturbed

motion are

$$\frac{d^2x}{dt^2} = -\frac{g}{l}x - \frac{dR}{dx},$$

$$\frac{d^2y}{dt^2} = -\frac{g}{l}y - \frac{dR}{dy};$$

and the variations of the four elements are given by the formulæ

$$\frac{da}{dt} = \frac{l}{g} \frac{a \frac{dR}{dr} + b \sqrt{\frac{g}{l}} \frac{dR}{d\alpha}}{a^2 - b^2},$$

$$\frac{db}{dt} = -\frac{l}{g} \frac{b \frac{dR}{dr} + a \sqrt{\frac{g}{l}} \frac{dR}{d\alpha}}{a^2 - b^2},$$

$$\frac{d\alpha}{dt} = \sqrt{\frac{l}{g}} \frac{a \frac{dR}{db} - b \frac{dR}{da}}{a^2 - b^2},$$

$$\frac{d\tau}{dt} = \frac{l}{g} \frac{b \frac{dR}{db} - a \frac{dR}{da}}{a^2 - b^2}.$$

As in the *Mécanique Céleste*, the perturbations are of two kinds,—the one periodic and producing no appreciable effect; and the other the so-called secular, and which, accumulating themselves, end, in virtue of their action repeated during a sufficiently long period, in producing an appreciable effect, however feeble the disturbing force. These latter it is important to calculate. In order to limit the question, we supposed that the perturbing force emanated from a fixed point A situated in the horizontal plane at a distance h from the centre O of the ellipse, and varied inversely as the square of the distance, which would be the case if there were an attractive or repulsive spherical mass fixed in this point. We supposed, moreover, that the initial value of the minor axis was null—that is, that the oscillation of the pendulum was originally plane. Under these conditions, it is easily seen that the secular part of $\frac{dR}{db}$ contains b as a factor; further, $\frac{dR}{d\tau}$ only contains periodic terms. It follows that $\frac{da}{dt}$ and $\frac{d\alpha}{dt}$ are small quantities of the second order (the perturbing force being taken as a small quantity of the first order), and therefore may be neglected. Hence the direction of the major axis and its magnitude are virtually invariable. But the two other elements undergo secular perturbations, and we get approximately

$$\frac{db}{dt} = -\frac{1}{a} \sqrt{\frac{l}{g}} \frac{dR}{d\alpha}, \quad \frac{d\tau}{dt} = -\frac{1}{a} \frac{l}{g} \frac{dR}{da}.$$

The variation of the element τ produces a change in the duration of the oscillation; but this small change did not seem to us capable of being determined with certainty by experiment, as the least change of temperature in a very long pendulum produces a much greater change in the duration of the oscillation. The only appreciable effect is the variation of the small axis.

The secular part of the perturbing function, making $b=0$, is represented by the formula

$$R = - \frac{f}{\sqrt{a^2 + h^2}} \sum_{i=0}^{i=\infty} \left[\frac{1.3.5 \dots (2i-1)}{2.4.6 \dots 2i} \left(\frac{a^2}{a^2 + h^2} \right)^i \times \right. \\ \left. \sum_{i'=0}^{i'} C_i^{i'} \frac{1.3 \dots (2i-4i'-1)}{2.4 \dots (2i-4i')} \left(\frac{h \cos \alpha}{a} \right)^{2i'} \right],$$

where f denotes the action of the perturbing force on the unit of mass at the unit of distance, the angle α being reckoned from the right line OA . In this sum the whole number i' varies from 0 to $\frac{i}{2}$ or to $\frac{i-1}{2}$, according as i is even or odd, and the number i from 0 to ∞ ; $C_i^{i'}$ denotes the number of combinations of i objects taken i' and i' . There is hence obtained

$$\frac{db}{dt} = - \frac{fh^2}{a^3 \sqrt{a^2 + h^2}} \sqrt{\frac{l}{g}} \sin 2\alpha \\ \times \sum_{i=2}^{i=\infty} \left[\frac{1.3 \dots (2i-1)}{2.4 \dots 2i} \left(\frac{a^2}{a^2 + h^2} \right)^i \times \right. \\ \left. \sum_{i'=1}^{i'} C_i^{i'} \frac{1.3 \dots (2i-4i'-1)}{2.4 \dots (2i-4i')} \left(\frac{h^2 \cos^2 \alpha}{a^2} \right)^{i'-1} \right].$$

The latter formula shows that the pendulum, which at first described a plane oscillation, afterwards describes an ellipse, the smaller axis of which increases proportionally to the time, and in a direction such that the velocity at the summit of the major axis is directed towards the point from which the perturbing force emanates if it is attractive, and in the opposite direction if it is repulsive.

The celebrated experiment of M. Foucault has shown that the plane of oscillation of a pendulum appears to be displaced in consequence of the motion of the earth. Let us suppose that the terminal ball is magnetic, and that on the two sides of the plane and at the two extremities of the oscillation magnets are placed; it will soon be observed that the pendulum describes an ellipse the major axis of which is displaced as in the experiment we have mentioned, and the minor axis of which increases proportionally to the time. If the positions of the two magnets be reversed, the small axis diminishes progressively, is annulled after the lapse of a time equal to that which had taken to form it, after which a new ellipse begins to form in the opposite direction. If the magnets are powerful and near, the increase of the small axis is seen at once; in proportion as

the force diminishes, the effect decreases; but as it is proportional to the time, it is only necessary to wait till it becomes appreciable and capable of being measured.

These very special conditions appear to us of a nature to justify the use of the pendulum for measuring attractive or repulsive forces, however small they may be, and for comparing them to a common unit, gravity. Those forces which are accessible to this mode of investigation are, in the first place, all the physical forces, and then especially that of universal attraction, which might show itself, not by a static effect of given magnitude, but by an action which is always accumulating so as to produce a considerable perturbation. Experiment has shown that all accidental causes may be eliminated which tend to alter the small axis of the ellipse, that the method is extremely sensitive, that it exhibits the feeblest electrical or magnetic actions. It will be understood that these researches will require a considerable time. They are already commenced; we shall successively publish the results.—*Comptes Rendus*, December 11, 1865.

ON THE EXPANSION OF SATURATED VAPOURS.

BY M. A. CAZIN.

Messrs. Rankine and Clausius deduced, in 1850, from the equations of the mechanical theory of heat the proposition that dry saturated aqueous vapour condenses partially on expansion, and that, conversely, it becomes superheated on compression, provided that adjacent bodies neither furnish nor withdraw from it any heat.

M. Hirn observed this about 1862. He verified, moreover, two other consequences of the same equations; that is, that bisulphide of carbon under ordinary circumstances behaves like water, and that ether behaves differently, becoming superheated by expansion, and condensing partially by compression.

M. Dupré has finally deduced from the equations of the theory the more general proposition, that for each liquid there is a temperature at which its saturated vapour might undergo an infinitely small expansion or compression with continued saturation; that at a lower temperature the expansion is accompanied by a condensation; at a higher temperature the contrary is the case. This follows from the relations established by M. Regnault between the total heats of vapours and the temperatures in his remarkable experiments.

I have been charged by the Physical Committee of the Scientific Association to verify this inversion. The apparatus constructed by M. Golaz has been set up in one of the rooms of the Observatory. In addition to M. Le Verrier, I wish to thank MM. Regnault and Hirn for their valuable advice.

The first researches were simply qualitative; before determining the temperature of inversion, I had to establish its existence.

The principal part of the apparatus consists of a cylinder 60 centimetres in length by 12 in diameter, with glass plates at its ends,

and immersed in an oil-bath. After being heated to a given temperature, it is exhausted and the liquid gradually introduced. The moment of saturation is ascertained by a slight deposit of dew on the glass plates. Connexion is then established with a cold reservoir containing air at a known pressure, lower than that of the vapour; and at the same time the condition of the cylinder is observed.

Water and ether behaved as in the experiments of M. Hirn; the vapour of the latter liquid never condensed by expansion, while that of the former always did. When the difference of pressure was more than 0·5 metre of mercury, the fog formed rendered the interior of the cylinder completely opaque; when it was less, a halo was often seen round a flame looked at through the vapour.

With chloroform, the inversion takes place when the pressure of the reservoir is increased. Beyond a certain pressure no condensation is obtained, even when the excess of the pressure of vapour is materially increased. An idea of the experiments may be formed from the following Table, in which the pressures are approximately measured by columns of mercury.

Pressure in the air-reservoir. metres.	Excess of pressure of the vapour. metres.	Temperature of the vapour. °	Effect observed.
0·75	0·90	85	Condensation.
0·75	1·09	89	"
0·75	1·62	99	"
1·47	0·92	99	"
1·47	2·18	117	"
1·84	2·01	119	"
2·25	2·52	129	"
3·27	1·13	125	No condensation.
3·50	1·10	127	"
3·94	2·50	143	"
4·01	2·64	145	"

It is thus seen that vapour saturated at 125° and expanding with an excess of pressure of 1·13 metre does not condense, but that vapour at 129° condenses with an excess of 2·52 metres. It is readily understood that this latter, attaining during expansion the temperature of inversion at the moment at which it has a pressure equal to the maximum tension which corresponds to this temperature, behaves from this moment like a vapour which starts from a temperature lower than that of inversion; thus in this case the fog is only visible at the end of the expansion.—*Comptes Rendus*, January 2, 1866.

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[FOURTH SERIES.]

MARCH 1866.

XXII. *Contributions to the Mineralogy of Nova Scotia.* By Professor How, D.C.L., University of King's College, Windsor, Nova Scotia*.

I.

IN the present series of communications I propose giving such analytical details and other information interesting to the mineralogist as have accumulated during several years' study of the minerals of Nova Scotia, and have not appeared in my previous papers†, and such as I may continue to obtain. In a succession of articles in the Transactions of the Nova Scotia Institute of Natural Science, the mineralogy of the province is being considered from an economic point of view ‡. In the last

* Communicated by the Author.

† These papers are the following :—

"On the Occurrence of Natroborocalcite in Gypsum of Nova Scotia" (Silliman's Journal, 1857).

"Analysis of Faroëlite and some other Zeolitic Minerals occurring in Nova Scotia" (Silliman's Journal, 1858).

"Analysis and Description of Three New Minerals from Trap of Nova Scotia" (Edinb. New Phil. Journ. 1859).

"On an Oil-Coal from Pictou Co., Nova Scotia" (Silliman's Journal, and Edinb. New Phil. Journ. 1860).

"On Gyrolite occurring in Trap of Nova Scotia" (Edinb. New Phil. Journ., and Silliman's Journal, 1861).

"On Natroborocalcite and another Borate in Gypsum of Nova Scotia" (Silliman's Journal, 1861).

"On Pickeringite occurring in Nova Scotia" (Quart. Journ. Chem. Soc. 1863).

"On Mordenite, a New Mineral from Trap of Nova Scotia" (Quart. Journ. Chem. Soc. 1864).

‡ "Notes on the Economic Mineralogy of Nova Scotia" (Trans. Nova Scotia Institute, parts 1 & 2).

Phil. Mag. S. 4. Vol. 31. No. 208. March 1866. N

of these I described the ores of manganese and their uses; and I select for the first subject in the present series of papers these ores, more particularly with reference to their chemical composition and mineralogical characters.

Manganite.—This species occurs abundantly at Cheverie, on the south shore of the Basin of Minas, in Hants Co., where it is found in nodules, sometimes of considerable size, on the beach, about twenty rods above high-water mark, and on the upland nearly two miles from the water. It is also met with at Walton, some miles to the east of Cheverie on the Petite River, where I have picked it up in the fields, and where a bed of it is said to crop out in a low hill on the river-side. It is mostly of compact crystalline structure, of a dark-grey colour, gives a brown streak and powder, and has hardness a little above 5. In the compact pieces small cavities are sometimes found, lined with black lustrous prismatic crystals affording a brown streak; these most probably consist of the essential elements of the species. The mineral is associated with barytes and calcite, and sometimes with pyrolusite. The geological formation of the surrounding district is lower carboniferous; the prevailing rocks are gypsum and limestone, sometimes containing magnesia, the latter being that in which I believe the ores of manganese always occur in this part of Nova Scotia.

A specimen from Cheverie was analyzed: the water and oxygen were determined by ignition in connexion with a tube filled with chloride of calcium; the amount of binoxide of manganese was ascertained by the oxalic-acid process, and the corresponding quantity of sesquioxide calculated; the siliceous gangue containing a small amount of barytes was estimated by action with hydrochloric acid as a solvent for the soluble constituents, among which were a little iron and baryta, which were not weighed. The results were these:—

Water	10·00
Sesquioxide of manganese* .	86·81
Gangue	1·14
Oxide of iron, baryta, and loss.	2·05
	<hr/> 100·00

The amount of oxygen lost on ignition was 3·57 per cent., and the theoretical loss, in the change of the Mn^2O^3 found into Mn^3O^4 , is 3·01. These numbers leave no doubt that the mineral is the hydrated *sesquioxide of manganese or manganite*, the theoretical composition of which is—

* Binoxide found = 47·73.

Calculation.			
MnO ²	. . .	43·57	49·43
MnO	. . .	35·57	40·36
HO	. . .	9·00	10·21
		88·14	100·00

for it is obvious that, if allowance were made for the unessential ingredients, there would be very close accordance. Another specimen of hard ore resembling the former, and probably from the same locality, gave a chocolate powder, and afforded

Binoxide of manganese, $45·4 = 82·4$ per cent. sesquioxide, with barytes and a little carbonate of lime,—results which prove it to be also manganite.

This species forms a curious bed of conglomerate, along with quartz pebbles, about ten miles to the west of Walton on the opposite shore of the estuary of the Avon, formerly considered to belong to the New Red Sandstone.

Pyrolusite.—This species is found at numerous localities in different parts of the province, and is now being mined in considerable quantity at one of them, viz. at Teny Cape, in Hants Co., about five miles from Walton, where about a thousand tons have been got out within the last two years, the bulk of which has been readily sold in England. It occurs here in the form of nodules of irregular, generally rather flattened shape, of all sizes, from that of a bean up to that of a man's head, or even twice as large, and weighing proportionately up to about twenty-five pounds. These masses lie loose in a bed of "soil" about a foot thick and a foot below the surface: they consist of pyrolusite and psilomelane. Some feet below this bed, in a grey and brick-coloured limestone containing magnesia, the ore is found, in very thin deposits, which, from the easily separable nature of the rock, can be laid bare in sheets, and also in "pockets" or interrupted chains of deposits of very variable dimensions, sometimes but a few inches in depth, and thickening out to several feet. I have seen one egg-shaped mass exposed *in situ* estimated to be of three tons weight. One of these "pockets," running east and west at a depth of 15 feet from the surface, was about 72 feet in length, varied in thickness from 6 inches to 14 feet, and was practically exhausted on the removal of about 130 tons of ore. A second runs parallel with this, at a depth of 30 feet from the surface, and has been found to extend at least 105 feet: it had yielded up to August last about 300 tons of ore; and a large quantity remained. Below this, again, at a depth of 50 feet from the surface, other deposits have been met with, the form and dimen-

sions of which have not, so far as I know, been fully made out, but which have afforded many tons of good ore. The whole thickness of the limestone holding manganese is estimated at about 300 feet.

The minerals associated with pyrolusite at Teny Cape are iron ore (brown hematite, I believe), barytes, and calcite. The first of these is occasionally found at the line of junction of the ore and rock, which, as before mentioned, is sometimes red. The barytes is of pure-white colour, is often disseminated in varying quantity through the pyrolusite, and is probably constantly present in all but the pure crystals of the species. The calcite is also occasionally imbedded, in transparent crystals, but more often exists as an incrustation; it sometimes forms specimens of great beauty, when it lies in opaque snow-white mammillary masses of finely-crystalline structure, or in piles of nail-head crystals, half an inch or an inch across, of grey or snow-white colour, on black lustrous masses of well-crystallized pyrolusite.

The pyrolusite found at Walton is sometimes attached to brown hematite in a reddish limestone resembling that at Teny Cape.

The forms of the mineral are various. It is generally highly crystalline. The masses at Teny Cape are sometimes of a grey black and consist of closely-packed fine long fibres, sometimes are made up of bunches of stellated short crystals, and often of distinct and lustrous jet-black crystals with perfect terminations: all these varieties yield readily to the knife. The Pictou ore (found at a distance of about seventy miles) is coarsely fibrous. The greater part of that from Walton is in soft, black, lustrous, short crystals; one specimen, however, has been met with almost crypto-crystalline in structure and of bluish-grey colour, closely resembling the ore from Saxony. A very similar specimen from Amherst, Cumberland Co., forty miles from Walton, gave on analysis in the air-dry state,

Water	0.61
Binoxide of manganese	97.04
Gangue and loss	2.35
	<hr/> 100.00

The insoluble matter (gangue) was brownish white, and most probably consisted of barytes.

I have no doubt that specimens of the greatest possible purity could be selected at Teny Cape. I have examined a good many samples of dressed ores, and have commonly found from 80 to 93 per cent. binoxide; a specimen obtained at a depth of 50

feet from the surface, taken as a sample of dressed ore, and weighing about a quarter of a pound, gave me in the air-dry state, in summer, 93·83 per cent. binoxide of manganese, with barytes and a mere trace of iron. It is a very valuable property of this ore, as regards its use by glass-makers, that when cleaned it contains remarkably little iron. The first shipment sent to England, consisting of about seven tons and a half, gave, on analysis in Liverpool, 91·5 per cent. binoxide, and less than a half per cent. of iron.

South of Teny Cape, at a distance of some ten miles, large nodules of manganese ore are found resembling in appearance those described as occurring in the "soil" at the former place. One of these weighed 180 pounds; a fragment from another, weighing thirty-five pounds, was examined by Mr. H. Poole, a pupil of mine. The mass was black, of unequal hardness, portions scratching apatite, and therefore about 5·5, while the rest yielded easily to the knife. The powder of the harder parts was nearly as black as that of the softer. The water of composition was found by weighing in chloride of calcium; the binoxide of manganese by oxalic acid; the results were these:—

Hygrometric water	1·660
Water of composition	3·630
Peroxide of iron	·603
Soluble baryta	·724
Insoluble (barytes?)	1·728
Binoxide of manganese	84·620
	<hr/> 92·965

which show that the mass consisted chiefly of pyrolusite. That the associated mineral was psilomelane follows from its appearance and hardness, the colour of its powder, and the amount of water contained, which is too little for manganite, and too much for any of the other manganese minerals.

The researches of MM. Deville and Debray (given in abstract in the 'Chemical News,' vol. i. p. 299) show that natural binoxide of manganese is a very complex substance, containing various soluble salts, among which are alkaline nitrates, and that nitric acid is one of its products of ignition. I have found the Teny Cape pyrolusite to give strongly acid fumes on ignition, no doubt from the presence of nitric acid. The nature of the soluble salts I have not inquired into further than regards iron and baryta. As a new illustration of the complexity of the mineral, it is an interesting fact that silver, to the amount of five ounces to the ton of ore, has been found in a specimen from Teny Cape, on assay by J. Taylor and Co., in London. I may here recall the fact that thallium to the extent of 1 per cent. has been detected by

Bischoff (Quart. Journ. of Science, October 1864, p. 688) in a specimen of pyrolusite from an unmentioned locality.

The majority of the localities affording pyrolusite in this province are almost certainly known to belong to the lower carboniferous beds; the country-rock of the ores has not in all cases been made known. I saw last summer, in a locality about five miles from the quartz and manganite conglomerate before mentioned, which may be of New Red Sandstone age, a hard highly siliceous rock, apparently quartzite (contiguous to slate), from which about a ton of ore, consisting of pyrolusite and psilomelane, had been recently taken.

Wad.—This is found in various parts of the province, sometimes in abundance. One specimen, of black colour, from a considerable bed situated, I believe, to the east of Halifax, gave me, when dried at 212° , 56 per cent. binoxide of manganese, a great deal of iron, a little cobalt, and a large quantity of insoluble matter. In specimens of brown "paints" I have found from 11 to 20 per cent. binoxide of manganese, the greater part of the residue being water and peroxide of iron.

XXIII. *On some Problems in Chances.* By J. M. WILSON, M.A.,
Fellow of St. John's College, Cambridge, and Mathematical and
Natural Science Master of Rugby School*.

THE problem of determining the probability that, if four points be taken at random in an infinite plane, one of the four shall lie inside the triangle formed by the other three, has now acquired some degree of notoriety. Various solutions have been given; $\frac{1}{2}$, $\frac{1}{4}$, $\frac{3}{8}$ have all been obtained as results; and all the methods are considered fallacious from introducing a comparison between infinities, and from those infinities apparently having different relative values according to the mode of approaching them.

I shall venture to offer a solution which depends on a different principle, and I shall first illustrate it by applying it to a simpler question.

To determine the probability that if three lines are drawn at random in an infinite plane, a fourth line drawn at random will intersect the triangle formed by the other three.

The peculiarity of this class of questions is that, if the triangle is supposed drawn, it must be finite in conception as compared with the infinities on all sides of it, and the chance required would appear to be indefinitely small. But since the first three

* Communicated by the Author.

lines are drawn *at random*, as the fourth line is, it is equally likely that all of the triangles should be infinite.

Let the four lines be drawn in any manner; call the lines *a, b, c, d*; *each of them in succession may be considered the fourth line*; and it will of course happen that in two of the four cases the fourth line does intersect the triangle formed by the first three; *and therefore the chance required is $\frac{1}{2}$.*

In this solution the italicized part contains the assumption, to which some attention should be given. It in fact *substitutes simultaneous for successive drawings* of the lines in the figure. Viewed in this light I do not think it can be objected to; for by hypothesis all the lines are at random and independent of one another; and this once granted, the rest of the solution presents no kind of difficulty.

It may be viewed as a compendious way of presenting the following solution. Let the lines be drawn in the order *a, b, c, d*. Among the infinite series of figures so obtained, it will necessarily follow that *d* and *a*, *d* and *b*, *d* and *c* will sooner or later change places, the others remaining where they were. Hence it is clear that the figures, if every possible figure could be conceived as drawn, could be arranged in groups of four, the *figures* being the same in each group, and *d* occupying each of the four positions of the lines in succession; that is, in each group *d* will twice intersect the triangle formed by *a, b, c*, and twice it will not; or the chance required is, as before, $\frac{1}{2}$.

It must be observed that wherever the first method of solution is applicable, it may be looked on as a succinct way of stating this kind of proof, if this latter is thought more satisfactory. The next problem is the "four-point problem."

If four points be taken at random in a plane, what is the chance that one of them lies inside the triangle formed by the other three.

(1) A point at random may be viewed as the intersection of two lines at random.

(2) Four random lines determine three sets of four random points.

(3) In one only of these three sets is one of the four within the triangle formed by the other three.

(4) And therefore the chance required is $\frac{1}{3}$.

With respect to (2), it may be remarked that if the lines are *a, b, c, d*, and *ab* represent the point of intersection of *a* and *b*, the three sets of four points are

ab, ac, db, dc;
ba, bc, da, dc;
ca, cb, da, db.

The steps by which this reasoning may be justified may be presented as follows :—

A blindfolded man is told that four points are marked on a large sheet, and is asked the chance that one of them is enclosed by the other three. He requests that the points may be joined by four lines, no three passing through a point. There are now six points; and if he is allowed now to inspect the figure, there are three original groups of four points from which it is equally probable that the figure originated, and in one only, and one always, of these three groups one point is enclosed by the other three; so that the probability is $\frac{1}{3}$.

The reasoning may also be justified in the way by which the result of the first question was arrived at.

This method, which might be called instantaneous, is easily applied to similar problems; and I have given these remarks in full, in order to remove, if possible, doubt as to its validity.

XXIV. *On the Level of the Sea during the Glacial Epoch in the Northern Hemisphere.* By Archdeacon PRATT.

To the Editors of the Philosophical Magazine and Journal.

Baitool, Central Provinces, India,
January 3, 1866.

GENTLEMEN,

DURING a journey in these out-of-the-way parts I have lighted upon an interesting letter in a late Number of the 'Reader' (September 2, 1865) by Mr. Croll of Glasgow, suggesting that the greater depth of the ocean in the upper parts of the northern hemisphere, during the Glacial Epoch, may have been caused by the change of the centre of gravity of the earth by the presence in the northern regions of an enormous accumulation of solid matter in an "Ice-Sheet," which has since disappeared. This idea appears to me so ingenious and so probable, that it deserves a more careful examination. The hypothesis appears to be, that in time past a grand cosmical change has been going on, according to which the northern and the southern hemispheres (at any rate the higher portions of them) have been alternately bound up in ice, and have alternately yielded to milder influences, when the ice-sheet has become broken up, moving off in huge fragments which have caused the phenomena of the drift, and has finally disappeared. The centre of gravity of the earth has therefore slightly shifted during these enormous periods, first north and then south, and produced a corresponding effect upon the depth of the ocean. The question is, whether the matter deposited from the air in snow and hail and held fast in a solid mass can have been sufficient to produce the required

rise in the sea-level. The problem is, in fact, one of Attractions. I have of late years given much attention to this subject in attempting to estimate the influence of the vast superficial mountain-mass north of India, and also of the deficiency of matter in the extensive ocean south of India down to the south pole, upon the plumbline used in the Trigonometrical Survey of Hindostan. The present problem is one of precisely the same character, as it is to ascertain the attracting force of the supposed Ice-Sheet upon the waters of the ocean and its effect in drawing them up. In the absence of all knowledge of the form and extent of the ice-mass, which has now disappeared, I will adopt Mr. Croll's hypothesis, as a means of making a calculation from which a general idea of the operation of the causes can be obtained. He takes the Ice-Sheet to be a hemispherical meniscus of a certain thickness at the pole, and gradually getting thinner towards the equator, where it is supposed to be zero. The specific gravity of ice is 0.92, and of superficial rock 2.75. Hence the ratio of that of ice to that of rock = 1 to 3.

2. Following the same course by which I have estimated the attraction of the Himalayan mass and of the deficiency of matter in the ocean, I have calculated that the Horizontal Attraction of a Spherical Meniscus of rock (of thickness h miles at the pole and radius a) at a place in the opposite hemisphere the polar distance of which is θ , measured from the pole of the attracting meniscus,

$$= (0.1446 \sin \theta + 0.0958 \sin 2\theta + 0.0244 \sin 3\theta) \frac{h}{a} g, \quad (1)$$

where g is gravity*.

From this formula it is not difficult to find the attraction of the meniscus upon a place on its own surface. For the tangential attraction of an oblate spheroid of small ellipticity ϵ , at a place on its surface of which the distance from the nearer pole is ϕ , $= 0.6 \sin 2\phi \cdot \epsilon \cdot g$, and acts *from* the nearer pole. From this it is easily seen, by taking the difference of effects of two oblate spheroids having the same equator and differing in their compressions by h , that the combined attraction of the northern and southern hemispherical meniscuses at the place in the north-

* See my treatise 'On the Figure of the Earth,' 3rd edit. p. 58. I avail myself of this opportunity to state that in two formulæ, (2) p. 59, and (6) p. 60, derived from the above formula, there is a misprint. It is not of much importance, as no use is made of those two formulæ. They should stand as follows:—

$$(0.1446 \sin \phi + 0.5042 \sin 2\phi + 0.0244 \sin 3\phi) \frac{h}{a} g \dots (2)$$

$$(2.0608 \sin \phi - 2.4884 \sin 2\phi + 0.7362 \sin 3\phi) \frac{h}{a} g \dots (6)$$

ern hemisphere

$$= 0.6 \sin 2\phi \frac{h}{a} g, \text{ and acts towards the north pole.}$$

Hence

$$\begin{aligned} 0.6 \sin 2\phi \frac{h}{a} g &= \text{horizontal attraction of northern} \\ &\quad \text{meniscus towards north pole} \\ &= (0.1446 \sin \theta + 0.0958 \sin 2\theta + 0.0244 \sin 3\theta) \frac{h}{a} g. \end{aligned}$$

Also $\theta = 180^\circ - \phi$. Hence

Horizontal Attraction of northern meniscus towards the north pole

$$= (0.1446 \sin \phi + 0.5042 \sin 2\phi + 0.0244 \sin 3\phi) \frac{h}{a} g.$$

3. Having thus obtained the force of attraction, I will find its effect on the level of the ocean on which the ice-sheet floats, wherever the ocean is not itself frozen. I may observe also that it is not necessary to suppose that the meniscus is altogether one unbroken solid mass. There may have been here and there comparatively small pools or lakes or canals of unfrozen water, in which the water would be lifted up by the attraction of the preponderating mass of solid attracting matter, and when so lifted up, nearly at any rate to the proper level, would itself attract nearly as much as the ice which would have taken its place had the whole been frozen.

If U is a force acting horizontally on the ocean at a point of which the distance measured along U 's line of action is u , then the rise of the ocean owing to this force

$$= \frac{1}{g} \int U du, \text{ between the proper limits*}.$$

For U put the expression above deduced; also $du = -d \cdot a\phi$. Then, integrating from the equator, where $\phi = 90^\circ$,

Rise of the Level of the Ocean

$$= \{0.1446 \cos \phi + 0.2521(\cos 2\phi + 1) + 0.0081 \cos 3\phi\} h \text{ miles.}$$

4. I will apply this to two latitudes, viz. 45° and 60° , or $\phi = 45^\circ$ and $\phi = 30^\circ$. These give as the rise $0.3486h$ and $0.5033h$ miles, if h is expressed in miles, and the attracting matter be rock; or $0.1162h$ and $0.1678h$ feet, if h be expressed in feet, and ice be the attracting matter.

Mr. Croll takes h to be 7000 feet. This gives the rise of the ocean in latitudes 45° and 60° to be 813 and 1175 feet.

5. These heights of the ocean above its present level may have

* See 'Figure of the Earth,' 3rd. edit. p. 156.

been somewhat greater, owing to the condition of the southern hemisphere at the time. For when the northern hemisphere had attained its maximum of glaciation, the southern hemisphere would have lost the ice-sheet in which it, in its turn, had been previously enveloped, and the released waters would have flowed, aided also by the attraction of the northern ice-sheet, from the southern hemisphere into the northern, and have correspondingly raised the general level of the ocean in the northern hemisphere above its previous state, and above the state to which it again came on the breaking up of the northern ice-sheet and the gradual re-formation of the ice-sheet in the south. To estimate the influence of this cause, we have but to make use of formula (1) in paragraph 2, and integrate from $\theta=0$. Then rise of the ocean from this cause

$$= -(0.1446 \cos \theta + 0.0479 \cos 2\theta + 0.0081 \cos 3\theta)h.$$

In latitudes 45° and 60° , $\theta=135^\circ$ and 150° ; and the rise of the ocean level $=0.0965h$ and $0.1042h$ feet, if h be expressed in feet, and the material be rock, or $0.0322h$ and $0.0347h$ feet, if the material be ice. Putting $h=7000$ feet, these become 225 and 243 feet.

6. It is evident that the southern ice-sheet is at the present time only very partially formed, and therefore cannot produce results near so large as those obtained in the last paragraph in lowering the present sea-level in the northern hemisphere.

Nor is it to be supposed that when the glaciation in the northern hemisphere was at its maximum, the ice-sheet reached down so far as the equator. The results obtained in paragraph 4 must therefore be somewhat diminished on this account.

We may put the change of level of the ocean in latitudes 45° and 60° north at, perhaps, 650 and 1000 feet.

7. But there is a circumstance well worth considering, which may have considerably increased these during the Drift Period.

I have shown that if the ice-sheet was a part of a hemispherical shell of thickness 7000 at the pole, and becoming thinner towards the equator, but stopping short in some latitude (say 30° or 35°) before reaching the equator, the difference of level of the ocean in latitudes 45° and 60° , in any opening connected by a channel, however narrow, with the unfrozen ocean about the equator, now and at the time of greatest glaciation in the northern hemisphere, may be taken at 650 and 1000 feet. The thickness of the ice in these latitudes would be 3500 and 5250 feet; that is, half and three-quarters of the thickness at the pole, because the thickness varies as the square of the sine of the latitude.

Now, as the ice-sheet began to melt and break up into por-

tions, the force of attraction would not change till the water from the melted ice had had time to flow away towards the open sea in the equatorial latitudes; for the water derived from the ice, though fallen down a few hundred feet into the channels and lakes below in the general mass, would attract very much as the ice itself did. And as the gradient of 1000 feet in 60° of latitude is very slight, the tendency of the water to flow out and of the sea-level to sink as the ice was turned into water would be feeble, and the outflow much obstructed by the windings of the channels and the countercurrents produced by impact upon the enormous ice-fields into which the ice-sheet would at first be broken.

It appears to me, then, extremely likely that the supply of water in the northern latitudes from the melting of masses between 3000 and 7000 feet high would be far more rapid than the outflow of water owing to the solid mass becoming less; and the consequence would be that during the Drift Period the ocean in the northern latitudes would stand at a much higher level, for a considerable time, than 650 and 1000 feet in latitudes 45° and 60° ; and vast icebergs with huge boulders fixed in their lower parts might find ample depth of water to carry them over all the places where the phenomena of abrasion and drift-deposit have been found.

J. H. PRATT.

XXV. *On some Minerals from Lake Superior.* By E. J. CHAPMAN, Ph.D., Professor of Mineralogy and Geology in University College, Toronto*.

A RECENT visit to the north-west shore of Lake Superior enabled me to obtain several minerals of much interest, including two or three species previously unrecognized in Canada. Brief descriptions of these latter, with a few observations on some of the other minerals which occur in this region, are offered in the following notes:—

1. *Native Lead.*—As a natural product, lead is well known to be of exceedingly rare occurrence in the simple or metallic state. On this continent—apart from its occurrence in the meteoric iron of Tarapaca in Chili—it has hitherto been noticed only at one spot, namely in a galena vein, traversing limestone (of unstated geological age), near Zomelahuacan, in the province of Vera Cruz, in Central Mexico. The specimen from the locality now under consideration was obtained at a spot near the celebrated Dog Lake of the Kaministiquia. The lead occurs in this specimen—the only one, I believe, discovered—in the form of a small string in white semiopake quartz. The quartz, which

* Communicated by the Author.

constitutes a narrow vein, does not appear to contain the slightest trace of galena, or any other substance, except a small quantity of specular iron ore; and the unaltered appearance of the latter is such as to preclude the supposition of the metal having been derived from galena, or other lead compound, by artificial heat. The lead, when cut, presents the ordinary colour, softness, and ductility of the pure metal. Before the blowpipe it melts readily, and volatilizes, imparting a blue tint to the flame-border, and forming a yellow ring of oxide on the charcoal. The fused globule is perfectly malleable. On the cupel it becomes entirely oxidized and absorbed without leaving a trace of silver. The cupel stain, when cold, is of a clear yellow colour, showing the absence of copper, nickel, &c. The nitric-acid solution yields with reagents the ordinary reactions of lead-oxide. The substance is distinguished from galena by its ductility, and by yielding no sulphur reaction with carbonate of soda before the blowpipe. From bismuth also it is distinguished by its perfect malleability, as well as by the blue colour which it imparts to the outer border of the blowpipe-flame. As a further test, it may be stated that a small cutting placed in a solution of bismuth in nitric acid produces a black arborescent precipitate of that metal.

This discovery is interesting, not only from the extreme rarity of *native lead*, but from the fact also that in the few undoubted European localities in which the metal has been found, the latter is generally accompanied by gold. The quartz in which the Lake-Superior specimen occurs has, curiously enough, the somewhat waxy aspect and other characters, more easily recognized than described, of the gold-bearing quartz of California and other auriferous districts; and the geological position of the bounding rock, immediately above that of the Huronian strata, is in a measure identical with the horizon of the gold-bearing rocks from which the auriferous deposits of Eastern Canada have been derived. No gold has hitherto been met with, however, in the sands of the Kaministiquia or other streams of Thunder Bay.

2. *Galena*, PbS.—This well-known mineral, the common ore of lead, occurs at numerous localities on the north shore of Lake Superior. Some especially rich lodes lie in the newly-surveyed township of Neebing, on Thunder Bay, and others of even greater promise have been discovered in the district around Black Bay. In most localities of this region the galena is accompanied by copper pyrites, the latter occasionally predominating. The vein-stone is principally quartz, with calc-spar, heavy spar, and fluor-spar in subordinate quantities. When crystallized, the galena presents almost invariably the common combination of cube and octahedron. This combination and the simple cube are the only

crystals that have come under my observation in these lodes. I have assayed a good many samples for silver without finding any workable quantity of the latter metal. The highest amount that I have obtained corresponds, indeed, to no more than $1\frac{1}{2}$ oz. to the ton of reduced lead. This comparative absence of silver appears to be connected with the very general absence of arsenical minerals throughout the district. A comparative study of the classical lead-districts of both Europe and this continent will, I think, be found to warrant the conclusion that, where arsenical ores, such as arsenical pyrites, Fahl-ores, &c., are generally absent, the galena will not prove to be argentiferous in a paying point of view.

3. *Marcasite*, FeS^2 .—The occurrence in Canada of iron pyrites in its trimetric or rhombic condition has not been hitherto announced. I obtained several well-characterized examples from the walls of a large vein, holding galena and copper pyrites, in lot 25 of the fifth concession of the township of Neebing, a few miles east of the Kaministiquia river. In all of these examples tabular prismatic crystals are united somewhat irregularly, but with the basal plane in common, in curved rows, with an acute angle of the prism projecting outwards, and thus forming the variety known as "Cockscorn Pyrites," the "Kammkies" of German authors. In this variety the crystals are not united regularly by a plane of the prism or by one of the macrodome planes, as in the true twins of marcasite, but are simply formed at the free end of the radiating lamellæ, the broad surface of the latter representing the basal plane. A point of much interest in connexion with these specimens is the occurrence of common or cubical pyrites *in the same vein*. The latter species occurs in different parts of this vein in small but distinct crystals—combinations of the cube and octahedron, with the cube faces predominating. Where representatives of the separate conditions of a dimorphous substance thus occur together, the cause by which the dimorphism was produced is not readily explained. In the present instance there were no data to show that one condition had originated at an earlier or later period than the other; and yet such must in all probability have been the case.

Some of the marcasite specimens from this spot had already entered into decomposition when first obtained, the products being an efflorescence of sulphur in one instance, and in others the formation of sulphate. The latter was also in itself altered by the partial conversion of the FeO into Fe^2O^3 , its solution yielding an abundant blue precipitate with ferrocyanide of potassium ("yellow prussiate").

4. *Molybdenite*, MoS^2 .—Several veins of quartz in which this mineral is abundantly distributed occur on the shore of Sea-beach

Bay, near Black River (lat. $48^{\circ} 46' N.$, long. $87^{\circ} 17' W.$). Some specimens from one of these veins gave me (by mechanical analysis) very nearly $4\frac{1}{2}$ per cent. of molybdenite, an amount equivalent to about 100 lbs. per ton. Copper pyrites is also present in the quartz.

5. *Barytine*, BaO, SO^3 .—It has long been known that many veins of heavy spar or barytine occur on the north shore of Lake Superior, several of these veins being almost free from colouring-matter, and hence of good quality as a paint-material; but I am not aware that any crystals from this region have hitherto been described. From the vein in Neebing township (about ten or twelve miles from Fort William), in which the cockscomb variety of marcasite (described in § 3) was obtained, I procured a great number of small crystals of this mineral of a pale yellowish or reddish colour. The same forms are present in all, producing a combination of (1) the base, $\infty \infty$; (2) a front polar or macrodome, $\frac{1}{4} \infty$; (3) a second or lower front polar, $\frac{1}{2} \infty$; and (4) the side polar or brachydome ∞ , the crystals being elongated in a right-and-left direction, *i. e.* in that of the macrodiagonal or longer horizontal axis. Most of the crystals, apart from this elongation, offer a very symmetrical aspect; but in some, as often happens, certain planes become crowded out, or reduced to mere lines, a plane of the form $\frac{1}{4} \infty$ being generally the sufferer. In the present case the angles measure as follows:— $\infty \infty$ (base): $\frac{1}{4} \infty = 158^{\circ}$; $\infty \infty : \frac{1}{2} \infty = 141^{\circ} 4'$; $\infty \infty : \infty = 127^{\circ} 15'$. Axes: a (vertical axis) = 1.315; $\bar{a} = 1$; $\bar{a} = 0.8141$. It should be observed, in reference to the crystallization of barytine, that some crystallographers make the base, as here given, a side vertical (or brachypinakoid of Naumann) = $\infty \infty$. In this position the front polars, $\frac{1}{4} \infty$ and $\frac{1}{2} \infty$, become vertical prisms; but the side polar or brachydome, ∞ , remains unchanged.

6. *Fluor-spar*, CaF .—Examples of this mineral are met with in many of the copper-ore and other veins of Lake Superior; but some unusually fine specimens have been lately obtained from large vugs in a broad vein of amethyst quartz, situated a few miles inland from the north-east corner of Thunder Bay. These specimens are crystallized in simple cubes, most of which measure from 2 to 3 inches across; and they occur as a bold capping on equally large pyramids of amethyst. The fluor-spar is thus the later formation of the two, and it is itself coated with a still newer formation of drusy pyrites in small cubes. Its colour is partly pale greenish, but mostly violet, like that of the chief mass of the quartz on which it lies. These fine crystals may be obtained in blocks of the dimensions of several cubic feet, forming magnificent museum specimens. Many of the amethyst crystals exhibit externally, or along their edges, a deep brownish

red colour, from the presence of innumerable spots of sesquioxide of iron deposited within or just beneath the surface-layer.

7. *Anthracite*.—In the Revised Report (1863) issued by the Geological Survey of Canada, a small amount of anthracitic matter is said to occur in cracks in the chert-beds of the lower division of the upper copper-bearing rocks of Lake Superior (altered strata of Lower Silurian age), as seen in the vicinity of Thunder Bay. A small vein of this kind, nearly vertical in position and averaging about 5 or 6 inches in width, occurs on the north shore of the bay. It was discovered by Mr. Herrick, a surveyor well known in Canada by his extended explorations in this remote district. A thin layer of colourless quartz lines the walls on each side. This is followed by about half an inch, or rather more, of iron pyrites, possessing a radiated structure, but crystallizing on its inner surface in combinations of the cube and octahedron. To this succeeds another band of white crystallized quartz; and the middle of the vein is filled with black and highly lustrous anthracite. The vein thus offers, though of small size, a fine example of banded or riband structure, showing, in passing from one wall to the other, (1) quartz, (2) iron pyrites, (3) quartz, (4) anthracite, (5) quartz, (6) iron pyrites, and (7) quartz. Here and there a thin coating of anthracitic matter occurs also on the surface of the pyrites, or runs through the latter, dividing it into two or more layers. So far as my observations go, all the large mineral veins of this district exhibit, on the other hand, a brecciated structure, with very subordinate or irregular indications of banding.

The anthracite from this vein possesses the following characters:—colour, jet-black with high lustre; streak, greyish black. Very brittle. Fracture, more or less conchoidal. $H = 2.25-2.5$. Spec. grav. (as determined by a light spec. grav. bottle) $= 1.43$. Before the blowpipe it cracks slightly and loses its surface-lustre, but exhibits no further change. Heated in a small flask or bulb-tube it gives off a little moisture, but without any accompanying trace of bituminous matter. In powder, in a thin platinum capsule, it burns completely away, but a long-continued ignition over a Bunsen's burner or double-current lamp is necessary to effect this. Carefully selected fragments do not leave a trace of ash.

Two assays gave the following results:—

Moisture	2.08	2.23
Additional loss by ignition in closed vessel	3.56	3.62
Ash	0.00	0.00
Fixed carbon, by difference	94.36	94.15

University College, Toronto, Canada West,
December 1865.

XXVI. *Studies on Gases.*

By Dr. H. W. SCHROEDER VAN DER KOLK.

[Concluded from p. 137.]

§ VI. *On the Difference of the Specific Heats.*

THE same formula which, if $\frac{c_1}{c}$ is known, gives the value of the mechanical equivalent of heat, can, conversely, be used for calculating $c_1 - c$ for different gases under different pressures and at different temperatures. $c_1 - c$ is then the unknown quantity in the formula

$$c_1 - c = \frac{\left(k + \tau \frac{dk}{d\tau}\right)^2}{J \left(k - p \frac{dk}{dp}\right)};$$

the determination of $\frac{dk}{d\tau}$ or $\frac{dk}{dp}$ and $\frac{dk}{dp}$ is effected by means of the formula previously found.

Hydrogen.

In the case of hydrogen, k , as has been observed, is independent of the temperature, and hence $\frac{dk}{d\tau} = 0$. The formula becomes therefore

$$c_1 - c = \frac{k}{J \left(1 - \frac{p}{k} \frac{dk}{dp}\right)};$$

$\frac{dk}{dp}$ is determined by means of the formula in § II. We have

$$\frac{dk}{dp} = \frac{dk}{13595 dh} = \{A + 2B(h-1)\} \frac{K}{13595},$$

in which A, B, and K have the values there given. We have

$$\begin{array}{ccc} 0.76 \text{ metre.} & 1 \text{ metre.} & 2 \text{ metres.} \\ c_1 - c & 1.00075, & 1.00093, & 1.00183. \end{array}$$

This difference increases, therefore, with the pressure, and, within the limits of the accuracy of Regnault's experiments, must be considered independent of the temperature. From this it follows that either c_1 or c , or both, change with the temperature.

Air.

In the case of air, $c_1 - c$ may be calculated for different pressures and temperatures. As regards the change of k with the
Phil. Mag. S. 4. Vol. 31. No. 208. March 1866. O

pressure, we have, in the first place, for 4° and 100° the formulæ given in the second paragraph; the analogous formula may also be calculated from the values of k at -87° given in § III. From the three values corresponding to the pressures 0.76, 1, and 2 metres, we get the formula

$$k = [0.99749 - A(h-1) + B(h-1)^2] K,$$

$$A = 0.0046608, \quad \log A = 7.66846 - 10,$$

$$B = 0.00017608, \quad \log B = 6.24570 - 10,$$

from which $\frac{dk}{dt}$ may be deduced for -87° .

Comparing this formula with the two corresponding to the temperatures 4° and 100° , it appears that the values of A and B , and therefore the variation from Mariotte and Gay-Lussac's law, are much greater at this lower temperature.

$c_1 - c$ may now easily be calculated for the pressures 0.76, 1, and 2 metres, and between the temperatures -87° and 100° . The formula is again

$$c_1 - c = \frac{\left(k + \tau \frac{dk}{d\tau}\right)^2}{J \left(k - p \frac{dk}{dp}\right)}.$$

For calculation this formula may be written in a more simple form. Putting $k = \psi K$, where ψ is the variable in the value k , and $p = 13595 h$, the formula becomes

$$c_1 - c = \frac{\left(\psi + \tau \frac{d\psi}{d\tau}\right)^2 K}{J \left(\psi - h \frac{d\psi}{dh}\right)}.$$

For 4° C. and 0.76 metre pressure we have in the case of air, for example,

$$K = 29.2443, \quad h = 0.76,$$

$$\psi = 1.0002995, \quad \frac{d\psi}{d\tau} = 0.0000060527,$$

$$\tau = 277.15,$$

$$J = 422.10, \quad \frac{d\psi}{dh} = -0.0012546,$$

from which is obtained $c_1 - c = 0.069470$. In the calculation Gauss's addition and subtraction Tables were always used, as given in Wittstein's Tables of Logarithms.

The following Table was obtained for $c_1 - c$ in the case of air:—

	0.76 metre.	1 metre.	2 metres.
-87°	0.070390	0.070633	0.071873
4	0.069470	0.069605	0.070132
100	0.069435	0.069512	0.069890

Hence the value of $c_1 - c$ decreases with increasing temperature and diminishing pressure. Taking for the limiting value of k , according to the Table in § III., $k = 1.00067$, which in any case is not far from the truth, though it cannot be quite definitely calculated, we have

$$c_1 - c = \frac{k}{J} = \frac{1.00067 \times 29.2443}{422.10} = 0.069328,$$

which gives the limit to which $c_1 - c$ approximates.

If this limiting value be calculated for hydrogen, it must, if reduced to the same volume, give the same number—as follows, indeed, from the well-known formula $c_1 - c = \frac{k}{J}$, which is applicable to an ideal gas, and hence also to this limiting condition. For $c_1 - c$ is proportional to k ; and the same applies to the volumes, since for the same weight, temperature, and pressure these vary with k . A direct testing was not possible, since the volumes determined by Regnault had to be reduced to the same limiting condition; and for this condition k , as was observed, could only be determined approximately.

Carbonic Acid.

In the case of carbonic acid this value could be calculated for 3° and 100° . We have, for instance, at 3° and 0.76 metre pressure,

$$\begin{aligned} K &= 19.0949, & h &= 0.76, \\ \psi &= 1.0020497, & \frac{d\psi}{d\tau} &= 0.000059951, \\ \tau &= 276.15, & \frac{d\psi}{dh} &= -0.008514. \end{aligned}$$

The following Table was thus obtained for $c_1 - c$ in the case of carbonic acid:—

	0.76 metre.	1 metre.	2 metres.
3°	0.04654	0.04677	0.04850
100	0.04572	0.04602	0.04699

In this case $c_1 - c$ may be readily calculated for the limiting condition from Gay-Lussac's law of volumes. The value of k for hydrogen in the limiting condition is very nearly = 422.23.

We have consequently for carbonic acid $k = \frac{422 \cdot 23}{22} = 19 \cdot 1923$, and, from the formula, $c_1 - c = \frac{k}{J} = 0 \cdot 045468$, towards which value the numbers of the Table also converge.

In the case of air and of carbonic acid, either c or c_1 , or both, change with the pressure and temperature.

§ VII. *On the Internal Work of Gases.*

Heat which is imparted to a body is in general used for three purposes—in increasing the temperature, and in internal and external work. Of these three, that which is changed into external work can be most easily determined; and if this quantity is subtracted from the total heat used, the sum of that applied to the other two purposes is obtained. Let us suppose a kilogramme of air at 4° and $0 \cdot 76$ metre pressure. Let the quantity of heat necessary to raise this through 1° C. under constant volume be c , the quantity used under constant pressure is $c + 0 \cdot 069470$. But in the latter case work is performed, the magnitude of which is easily determined. Under constant pressure the formulæ before and after the heating are

$$pv = k\tau, \quad pv_1 = (\tau + 1) \left(k + \frac{dk}{d\tau} \right),$$

from which is obtained

$$p(v_1 - v) = k + \frac{dk}{d\tau} (\tau + 1).$$

The external work done is therefore in thermal units

$$= \frac{1}{J} \left\{ k + \frac{dk}{d\tau} (\tau + 1) \right\}.$$

In this case k and $\frac{dk}{d\tau}$ must be calculated from the formulæ previously obtained for $0 \cdot 76$ metre and 4° ;

$$\tau + 1 = 273 \cdot 15 + 5 = 278 \cdot 15,$$

and J is the mechanical equivalent of heat. This quantity of heat is found to $= 0 \cdot 069420$.

Hence if a kilogramme of air, under a constant pressure of $0 \cdot 76$ metre is heated from 4° to 5° , the excess of heat which the air contains at 5° over that which it contains at 4° is

$$= c + 0 \cdot 06947 - 0 \cdot 06942 = c + 0 \cdot 00005.$$

In heating under constant volume, the heat c is to be added, by which the pressure increases from $0 \cdot 76$ metre to

$$\frac{278}{277} \times 0 \cdot 76 = 0 \cdot 7626.$$

The quantity of heat in a kilogramme of air at 4° and 0.76 metre pressure is less than that which it contains

at 5° and under the pressure 0^m.7626 by c ,

and

at 5° „ „ 0^m.76 by $c + 0.00005$.

Hence in the latter case more heat is contained in the air than in the former.

If a kilogramme of air at 5° , and under a pressure of 0.7626 metre, were allowed to expand until the pressure was 0.76 metre without performing any external work, as, for instance, if it were placed in connexion with an exhausted space, 0.00005 thermal unit would have to be added in order to keep the temperature at 5° . If this quantity is not added, the temperature sinks. In this way the following results are obtained:—

Hydrogen.

Pressure. _m	$c_1 - c$.	External work.	Difference.
0.76	1.00075	1.00047	0.00028
1	1.00093	1.00056	0.00037
2	1.00183	1.00098	0.00095

Air.

$t = -87^{\circ}$.

0.76	0.070390	0.069918	0.000472
1	0.070633	0.070034	0.000599
2	0.071873	0.070633	0.001240

$t = 4^{\circ}$.

0.76	0.069470	0.069420	0.000050
1	0.069605	0.069488	0.000117
2	0.070132	0.069748	0.000384

$t = 100^{\circ}$.

0.76	0.069435	0.069382	0.000053
1	0.069512	0.069424	0.000588
2	0.069890	0.069613	0.000277

Carbonic Acid.

$t = 3^{\circ}$.

0.76	0.04654	0.04608	0.00046
1	0.04677	0.04620	0.00057
2	0.04850	0.04705	0.00145

$t = 100^{\circ}$.

0.76	0.04572	0.04562	0.00010
1	0.04602	0.04577	0.00025
2	0.04699	0.04628	0.00071

The numbers of the last column show by how much the gases deviate from Mayer's assumption. From these numbers the formula may be calculated which gives the quantity of heat which must be imparted to, or taken from a gas, if, while it retains a constant temperature, it is to expand from a higher to a lower pressure without performing any external work. We can always imagine here a globe filled with gas, which is put in connexion with an exhausted globe. Let us calculate the formula for air at 5° . At this temperature let c , c' , and c'' denote the specific heats under constant volume for the pressures 0.76, 1, and 2 metres; we have then, at 5° ,

Pressure.	
0.7626	c ,
0.7600	$c + 0.000050$,
1.0035	c' ,
1.0000	$c' + 0.000117$,
2.0070	c'' ,
2.0000	$c'' + 0.000384$.

Different values of c are given, as it is not to be presupposed that c is independent of the pressure. Yet in calculating the formula this has no influence.

The numbers in the last column show how much more heat is contained in a kilogramme of air at 5° and the pressure given, than in one at 4° and the corresponding pressures 0.76, 1, and 2 metres.

From this the three following differential quotients of the internal work w may be deduced:—

$$\text{At } 2^{\text{m}}, \quad \frac{hdw}{dh} = \frac{2 \times 0.000384}{-0.0070} = -0.110,$$

$$\text{at } 1^{\text{m}}, \quad \frac{hdw}{dh} = -0.031,$$

$$\text{at } 0^{\text{m}}.76 \quad \frac{hdw}{dh} = -0.014.$$

The increase of pressure dh is not given in metres, but in parts of the existing pressure. In this way the constants of the following formulæ were calculated,

$$h \frac{dw}{dh} = \alpha - \frac{\beta}{\gamma - h},$$

and they gave

$$\alpha = 0.798; \quad \beta = 9.535; \quad \gamma = 12.5.$$

Hence

$$\frac{dw}{dh} = \frac{\alpha}{h} - \frac{\beta}{(\gamma-h)h},$$

from which is obtained, by integration,

$$w = \left(\alpha - \frac{\beta}{\gamma}\right) \log \text{nat } h + \frac{\beta}{\gamma} \log \text{nat } (\gamma - h) + \text{const.};$$

or between the pressures h_1 and h_0 , changing the natural logarithms into common ones,

$$w = \frac{1}{M} \left(\alpha - \frac{\beta}{\gamma}\right) \log \frac{h_1}{h_0} + \frac{\beta}{M\gamma} \log \frac{\gamma - h_1}{\gamma - h_0},$$

in which $M=0.4342$ gives the modulus. The introduction of the coefficients gives

$$w = 0.0806 \log \frac{h_1}{h_0} + 1.7564 \log \frac{12.5 - h_1}{12.5 - h_0},$$

where h_1 denotes the lower, and h_0 the higher pressure.

For $h_1=1$ and $h_0=2$ metres, we have

$$w = -0.0243 + 0.0693 = 0.0450 \text{ thermal unit.}$$

Hence with two equal globes, one of which is exhausted and the other filled with 1 kilogramme of air at 5° and 2 metres pressure, if the latter is allowed to flow into the former, 0.045 thermal unit must be added in order to keep the temperature at 5° . If this is not the case, the gas is cooled; and if the specific heat under constant volume = 0.1681, the cooling amounts to

$$\frac{0.0450}{0.1681} = 0.27^\circ \text{ C.}$$

In like manner this may be calculated for the other gases. I have not, however, repeated this more complete calculation, since the value for present use may be obtained with sufficient accuracy by means of simple interpolation. Calculating in the above case the value from the first column

$$= 0.0035 + \frac{1}{3}(0.0070 - 0.0035) = 0.0046,$$

and dividing this by the corresponding value of the second column

$$= 0.00017 + \frac{1}{3}(0.000384 - 0.00017) = 0.00026,$$

we obtain 0.0449, which is almost exactly equal to the value calculated directly.

The following numbers were calculated in this way. They always refer to the case in which a vessel containing a kilogramme of gas under 2 metres pressure is placed in connexion with an exhausted one of the same capacity. The first column gives the

temperature, the second the heat to be added, and the third the fall in temperature when no heat is added.

Hydrogen.					
Temp.	Addition of heat. Unit.	Depression of tempe- rature.	Thomson and Joule.		Temp.
			Difference of pressure.		
			0.76 metre.	1 metre.	
	0.120	0.05	0.02	0.03	°
Air.					
-87°	0.178	1.08			
4	0.043	0.27	0.26	0.34	17
100	0.033	0.20			
Carbonic acid.					
3	0.187	1.21	1.20	1.58	11
100	0.087	0.56	0.75	0.92	91

Thomson and Joule have, as we know, determined the depression of temperature which takes place when a current of gas is pressed through a narrow aperture, and have thus obtained the results given in the last column. The fourth column gives the depression of temperature which takes place at the temperature given in the sixth column, when the difference of pressure is an atmosphere. Comparing these numbers with those in the third column, we see that they are tolerably proportional one to another, especially if we take into account the differences in temperature; in the case of carbonic acid Thomson's number refers to 91°, the preceding to 100°. This agreement in results obtained in such entirely different ways may be regarded as a confirmation of the theory.

The numbers in the third and fourth columns cannot be directly compared, since they refer to unlike differences of pressure. The fifth column gives the cooling in Thomson and Joule's experiments for a difference in pressure of a metre. Comparing this and the third column, it is found that the cooling in the direct experiments was always greater except in the case of hydrogen; but the absolute value in numbers is in this case too small to consider this an exception and not an error of observation.

Yet a cause may be adduced for the constant difference in the case of air and carbonic acid. The cooling observed by Thomson and Joule arises from three causes:—the internal work performed in the passage from the initial to the final condition; the work which is performed in imparting to the issuing gas

the constant greater velocity; and, thirdly, the friction, in which heat is indeed again produced, but which, as Clausius shows*, partly passes into the sides of the vessel. In our calculation for the case in which the gas issues from a globe into an exhausted one, the first cause only requires to be taken into account; Thomson and Joule's experiments, in which all three causes cooperate, must in any case give a greater cooling, as indeed the above numbers show.

We might, indeed, consider the difference of the values in the third and fifth column to be the cooling arising from the two latter causes; I think, however, that these numbers have indeed a qualitative, but only a very small quantitative value. The question here is as to the difference of two very small magnitudes; and even if in Thomson and Joule's case the absolute error in the determination of temperature is very small, it might yet relatively be considerable.

The numbers of the third column are only calculated from Mariotte and Gay-Lussac's law, but these might be calculated much more accurately from Regnault's experiments. In the first place, the values are obtained by means of rough interpolation; a more accurate calculation would not be of much use, on account of the cause given in the fourth section. The formula given in § II. for carbonic acid is manifestly only approximate, since the coefficient of $(h-1)^2$ is found here to be greater than at 3° , which cannot be the case if gases generally approximate to the real condition with increasing temperature. It follows from this that the differences of specific heats, in the case of carbonic acid, change regularly, yet that the differences of these series have not the desired continuity. It would have been better to calculate the formula for carbonic acid at 100° from the coefficient of expansion by means of the method of least squares. But all this only gives greater accuracy in case the fundamental formula of Mariotte's law deduced from Regnault's investigations is more accurate than the one which occurs in his memoir.

It follows, further, from the theoretically deduced formulæ, that Thomson and Joule's result, that the cooling is proportional to the difference of pressure, is inaccurate. But the deviations are too small to be capable of experimental determination.

It is interesting to remark that an ideal gas, one therefore to which Mayer's hypothesis applies, would undergo no cooling in the experiment with the globes, but would in Thomson and Joule's experiments; for in an ideal gas the first cause for cooling does not exist, though the two others do, as is easily intelligible.

* Clausius, *Abhandlungen über der mechanischen Wärmetheorie*, p. 107.

§ VIII. *Conclusion.*

The following remarks may be made in conclusion. In defining an ideal gas, in § I. only has it been assumed that k is constant. This is enough where it is only a question of deviations from Mariotte and Gay-Lussac's law; and the deduction of the absolute zero depends on no further assumption. But where the specific heat is concerned, it must be defined more accurately whether the gas in expanding is to perform internal work or not. The assumption that in this gas no internal work is performed is at the basis of the definition of Carnot's function of temperature. Upon this depends the further development of the theory, and its application to bodies occurring in nature.

An ideal gas, therefore, must satisfy the three following conditions: k is independent of pressure and temperature, and performs in expanding no internal work. Now Cohen-Stuart has shown* that the second condition follows from the first and third; the assumption, therefore, that a gas satisfies the first and third but not the second, is self-contradictory. If there were an ideal gas, the question would soon be settled; but as this is not the case, the question is, whether an ideal gas as defined above can occur; or whether perhaps it is not in contradiction with other natural laws, whether known or unknown to us. If the mechanical theory of heat is to be developed in accordance with fact and not as a mere play of formulæ, it is of the greatest importance to investigate this as accurately as possible.

It frequently occurs that in an analytical investigation a new magnitude is introduced to simplify calculation; this obtains its significance from its definition, and is without influence on the results of the theory. This is the case, for instance, with the potential function. Now the assumption of an ideal gas materially simplifies the development of the mechanical theory of heat; but in the present case this is neither sufficient nor is it the chief question. If the theory is to be developed in accordance with fact, it must start from premises which occur in nature, and not from such as are arbitrarily excogitated by us. Hence the ideal gas is only another name for the condition to which, according to our idea, one or more gases approximate. The occurrence of this condition has never been experimentally proved, and it is only from the known properties of gases that we can conclude with greater or less certainty that there is such a condition. We know that at a low pressure and high temperature the deviations from the ideal condition are small, but this is by no means adequate for a firm establishment of the theory. It may readily be imagined that in applying this theory to gases

* Poggendorff's *Annalen*, vol. cxix. p. 327.

a small deviation is of very small importance, but of much greater in the case of solids and liquids. It is therefore of urgent necessity to investigate as accurately as possible whether such an ideal state occurs in the case of gases—not because the condition of these gases is of such importance in itself, but because the entire theory of heat, so far as it is connected with the ordinary definition of Carnot's function of temperature, stands or falls with it.

The occurrence of this ideal condition is the more probable, the more the various deviations of actual gases from the ideal condition taken together converge towards *nil*. As regards Mariotte and Gay-Lussac's law, this follows from Regnault's investigation, and it is confirmed by the testing of the law of volumes; the values of the internal work given in § VII. show, too, that this decreases with higher temperature and lower pressure; this also follows from Thomson and Joule's experiments, which in this respect have great value. So far as previous observations permit, I believe I may conclude, from what has here been communicated, that these deviations decrease *together*, by which the validity of the assumption of an ideal gas acquires a confirmation.

Zütphen, February 1864.

XXVII. *On the Black-bulb Thermometer.*

By PROFESSOR TYNDALL, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the February Number of the *Philosophical Magazine*, Mr. Wilson of Rugby has published an interesting letter, in which he describes an observation made by Mr. Glaisher, to the effect that the difference of reading between a black-bulb thermometer exposed to direct sunshine, and a second thermometer shaded from the sun's direct action, diminishes as we ascend in the atmosphere. This observation points to the following conclusions which have been drawn by Mr. Wilson.

1. "If the experiment could be made, the shaded and exposed thermometers outside the earth's atmosphere would show the same very low reading."

2. "All those views of the state of the surface of the moon which represent it as alternately exposed to fiery heat and intense cold will disappear. The temperature on the dark and bright halves of the half moon is the same if there is no atmosphere."

"We learn," continues Mr. Wilson, "one more marvellous function of our atmosphere before unsuspected. We knew that it and the aqueous vapour it bears diffused through it save us from loss of heat. We did not know that but for it, or some part of it, we should have no heat to lose. Hence, too, we learn that conclusions as to the temperatures of other planets are still more open to uncertainty, as being in this way also influenced by their atmospheres."

These are very weighty inferences. They affirm that what is heat in our atmosphere, is not heat in stellar space; that something is emitted by the sun which warms a body surrounded by an atmosphere, but which is powerless to warm the body if not so surrounded. A planetary atmosphere, in short, has a power of transmuting into heat an agency which, prior to its entering the atmosphere, is not heat, and this power of transmutation, if possessed in a very high degree, might raise the most distant planet to a high temperature.

I must frankly confess that I do not consider these conclusions, however fairly drawn, to be representative of natural facts. I am ignorant of the details of Mr. Glaisher's observation, and therefore not in a position to offer any explanation of it. But the following remarks, which are in substance transferred from a paper presented some time ago to the Royal Society, and ordered to be printed in the *Philosophical Transactions*, may have some bearing upon the question; or if not, they may bring to the notice of meteorologists a possible defect in their observations on solar radiation, which, as far as I know, has been hitherto overlooked:—

1. Solar heat as it reaches us consists partly of visible and partly of invisible rays, a portion of the latter possessing very high calorific power.

2. The ordinary black-bulb thermometer absorbs the visible rays of the sun, but the black glass of the bulb may be highly transparent to the invisible radiation.

3. The invisible calorific rays, especially, augment in power as we ascend in the atmosphere; for it is those rays which suffer most diminution of intensity during their passage through the aqueous vapour of the air.

4. Hence the black-bulb thermometer must, with reference to the total radiation falling upon it, become more and more transparent as we ascend in the atmosphere. It is not an exaggerated estimate that at the limits of our atmosphere 50 per cent. of the solar heat might cross the glass of the bulb, be reflected by the mercury within it, and contribute in no degree to the heating of the thermometer.

If these remarks be correct, or so far as they are correct,

the indications of the black-bulb thermometer, as at present constructed, are delusive, and they are especially so at great elevations.

I am, Gentlemen,

Your obedient Servant,

Royal Institution,
February 14, 1866.

JOHN TYNDALL.

XXVIII. On Archdeacon Pratt's '*Figure of the Earth.*'

By Captain A. R. CLARKE, *Royal Engineers, F.R.S., F.R.A.S.**

IN the application of the method of least squares to the determination of the figure of the earth, there is this anomaly, that the semiaxes are not determined by making the sum of the squares of the errors of observation or measurement a minimum. Of the existing uncertainty as to the figure of the earth, but a small portion is due to errors of measurement or observation; it is mainly due to the circumstance that the earth is irregular, and cannot be precisely represented by any ellipsoid or other simple (algebraically speaking) surface. Imagine a spheroid having its axis coincident with that of the earth's rotation, and actually intersecting the mathematical surface of the earth in various curves; if P be any point in the former surface, p that in which the normal at P meets the irregular surface, then, as far as our present knowledge extends, and for values of the axes between certain limits, Pp may be everywhere a very small quantity, and we become aware of its existence only through its variations. These appear as local displacements of the zenith, or, in other words, discrepancies in the latitudes and longitudes of astronomical stations. Now these latitude-discrepancies, or, as they are otherwise expressed, deflections or local attractions, are in their average magnitude very much larger than the probable errors of the astronomical determinations of latitude, and indeed overwhelm the errors of the geodetical operations. Moreover the local deflections become, when we attempt to ascertain the figure of the earth, inseparably mixed up with the errors of measurement and observation. Hence to determine the figure of the earth from a number of measured arcs and observed latitudes, we have to find that spheroid which, when the measured arcs are, as it were, applied to it, shall give the sum of the squares of the deflections an absolute minimum; in other words, corrections x_1, x_2, \dots have to be applied to the observed latitudes of the points P_1, P_2, \dots such as to bring them into accordance with the spheroidal surface, while the sum of the squares of the quantities x_1, x_2, \dots is to be an absolute minimum. The spheroid

* Communicated by the Author.

being determined, x_1, x_2, \dots are the local deflections at the points $P_1, P_2 \dots$ with reference to that spheroid.

All this seems exceedingly simple; but Archdeacon Pratt, in the third edition of his 'Treatise on Attractions, Laplace's Functions, and the Figure of the Earth,' of which a notice appeared in the last Number of the Philosophical Magazine, has obscured it by attaching some special importance to certain points (one in each arc) which he calls *standard stations*, a term unknown to geodesists. For instance, St. Agnes is considered the standard station of the English arc; but on what grounds? because it is the southernmost station? If so, Saxaford has an equally good claim to be considered the standard station because it is the northernmost; and Greenwich Observatory might have a special claim. But in reality the standard station is that with reference to which the differences of latitude in each arc have been taken. The selection of the point in any arc with reference to which differences of latitude are to be taken is merely a matter of arithmetical convenience—nothing more: the southern extremity (in the northern hemisphere) presents facilities; so does the northern; but it is really quite immaterial, since the result, as is easily shown, is quite independent of this consideration. Any station, then, in an arc may be selected as the standard station in this sense.

If x_1, x_2, \dots be the corrections to the latitudes of (or local attractions, or deflections at) the reference stations in each arc, the corrections to the latitudes of the other stations will be

$$\text{1st arc. } m_1 + \alpha_1 u + \beta_1 v + x_1, \dots m'_1 + \alpha'_1 u + \beta'_1 v + x_1, \dots$$

$$\text{2nd arc. } m_2 + \alpha_2 u + \beta_2 v + x_2, \dots m'_2 + \alpha'_2 u + \beta'_2 v + x_2, \dots$$

$$\text{3rd arc. } m_3 + \alpha_3 u + \beta_3 v + x_3, \dots m'_3 + \alpha'_3 u + \beta'_3 v + x_3, \dots$$

and the sum of the squares of these quantities $+x_1^2 + x_2^2 + \dots$, is to be a minimum; differentiating this quantity with reference to $u, v, x_1, x_2, x_3 \dots$, Archdeacon Pratt says, "This mode of proceeding is, I conceive, erroneous, as I shall now endeavour to show. The corrections $x_1, x_2 \dots$ are not properly independent variables, but are functions of u and v , and of the deflections produced by local attraction." Then follows a demonstration in which the point seems at first sight to be proved; but in the demonstration it is tacitly assumed (see figure, page 259 of No. 64, vol. xiii. of the Proceedings of the Royal Society, or page 128 of Archdeacon Pratt's work, third edition) that the southern extremity A, or reference-point, of the arc AB is a *fixed point*. What is there to fix it? Nothing is known as to the actual position of the arc AB with reference to the semiaxes of the meridian ellipse; what we know about the arc is simply its length AB, and the inclination, to the axis of rotation, of the

tangents at A and B. These alone are fixed ; so long, then, as the curve moves parallel to itself, its position is arbitrary. In fact its position is determined by the best way in which it can accommodate itself to the hypothetical ellipse, changing with every variation of the variable ellipse.

Imagine an arc of, say, four astronomical stations A, B, C, D ; then we know the lengths AB, AC, AD, and we know the directions of the tangents at A, B, C, and D; this is all. As for the local attractions at these astronomical stations, they cannot in any case be pronounced upon even with carefully contoured plans of the ground ; for the effect of irregularities which exist in the density of the crust of the earth cannot be ascertained. The local attractions at A, B, C, D can only be inferred by comparing this arc with received elements of the figure of the earth. Suppose that only one allowable value of the semiaxes exists, and let the ellipse be described ; the curve ABCD must then (retaining its fixed *direction*) be shifted about until it coincides as closely as possible with some portion of the ellipse. If it can be so managed that ABCD can be placed against other points *a, b, c, d* of the ellipse so that the tangents at A and *a*, B and *b*, C and *c*, D and *d* are respectively parallel, then we conclude that there exists no local attraction at any of the stations, or that it is equal and in the same direction at all the points—a less probable hypothesis. If parallel tangents cannot be found on the ellipse, then the arc must be so adjusted that the sum of the positive angles included by the corresponding tangents shall be equal to the sum of the negative angles. In other words, the algebraic *sum* of the apparent residual local attractions at A, B, C, D must be zero. This is merely the principle of the arithmetic mean. Now make a different supposition as to the semiaxes, and we have a different ellipse. Fitting the same arc to this as before, we get another system of most probable deflections differing from the former, but whose sum is also zero.

For every hypothesis as to the figure of the earth there exists then a corresponding most probable system of deflections for the astronomical stations on any arc. But Archdeacon Pratt immovably fixes the southern point or standard station of his arc, and so falls into error. One of the erroneous results of this method of calculation may be quoted : “for example, a deflection of the plumbline of only 5'' at the standard station (St. Agnes) of the Anglo-Gallic arc would introduce a correction of about one mile to the length of the semi-major axis, and more than three miles to the semi-minor axis.” Now if there *had* been any such disturbance of the plumbline at St. Agnes, the method of least squares would without fail have found it out, and the corresponding error introduced in the semiaxes would have been only a few

feet—45 feet and 10 feet, instead of a mile and three miles respectively. This result is readily obtained from the formulæ at page 769 of the 'Account of the Principal Triangulation.'

Alterations in the Semiaxes of the Earth (given at p. 771 of the 'Account of the Principal Triangulation') which would result from an alteration of 1" in the latitudes of the different Astronomical Stations.

	$\delta a.$	$\delta b.$		$\delta a.$	$\delta b.$
<i>Anglo-Gallic Arc.</i>	feet.	feet.	<i>Russian Arc.</i>	feet.	feet.
Formentera.....	-33.9	-42.4	Staro-Nekrassowka ...	-37.5	-17.7
Montjoux	-29.1	-30.1	Wodolui	-34.4	-12.4
Barcelona	-29.1	-30.1	Ssuprunkowzi.....	-28.9	-7.5
Carcassonne	-25.4	-22.6	Kremenetz	-25.2	-4.3
Pantheon	-11.8	-4.6	Belin	-19.4	-0.2
Dunkirk	-5.6	+ 0.5	Nemesch.....	-11.1	+ 3.9
St. Agnes	-9.0	-2.0	Jacobstadt	-4.8	+ 6.0
Goonhilly	-8.5	-1.7	Dorpat	+ 2.0	+ 7.4
Hensbarrow	-7.5	-0.9	Hogland	+ 8.5	+ 8.0
High Port Cliff	-6.9	-0.4	Kilpi-Maki	+18.6	+ 7.8
Week Down	-6.9	-0.4	Tornea	+32.1	+ 6.0
Boniface Down	-6.9	-0.4	Stuor-Oivi	+44.9	+ 2.9
Dunnose	-6.9	-0.4	Fuglenæs.....	+54.2	0.0
Black Down	-6.7	-0.2			
Southampton	-6.0	+ 0.2	<i>Indian Arc, II.</i>		
Greenwich Observatory.	-4.3	+ 1.4	Punnæ	+ 3.1	-82.9
Precelly	-2.9	+ 2.3	Putchapalliam	+ 1.3	-56.0
Cambridge Observatory.	-2.1	+ 2.8	Dodagoontah	+ 0.2	-37.4
Arbury Hill	-2.1	+ 2.8	Namthabad.....	- 0.6	-18.4
Delamere	+ 1.1	+ 4.5	Damergida	- 1.5	+ 7.7
Clifton Beacon	+ 1.8	+ 4.9	Takal Khera	- 1.9	+33.3
South Berule	+ 4.0	+ 5.9	Kalianpur	- 1.5	+57.3
Burleigh Moor	+ 5.4	+ 6.5	Kaliana	+ 1.0	+96.3
Durham	+ 6.1	+ 6.8			
Ben Lomond	+10.9	+ 8.4	<i>Indian Arc, I.</i>		
Kellie Law	+11.1	+ 8.4	Trivandeporum	+ 0.4	- 7.3
Ben Heynish	+11.9	+ 8.6	Pandree	- 0.4	+ 7.3
Great Stirling.....	+15.5	+ 9.5			
Monach	+18.7	+10.0	<i>Prussian Arc.</i>		
Ben Hutig	+19.5	+10.1	Truntz	- 2.2	- 0.8
North Rona	+21.6	+10.4	Königsberg.....	- 0.6	- 0.2
Balta	+27.9	+10.8	Memel	+ 2.8	+ 1.0
Gerth of Scaw	+28.1	+10.8			
Saxaford	+28.2	+10.8	<i>Peruvian Arc.</i>		
			Tarqui.....	+ 1.2	-15.3
			Cotchesqui	- 1.2	+15.3
<i>Danish Arc.</i>					
Lauenberg	- 1.1	- 2.0	<i>Hanoverian Arc.</i>		
Lysabbel.....	+ 1.1	+ 2.0	Göttingen	- 3.0	- 1.9
			Altona.....	+ 3.0	+ 1.9

In conclusion, the elements of the figure of the earth deduced by Archdeacon Pratt are, although they happen to be near the truth, arbitrary results, founded on an incorrect calculation, and cannot be taken to supersede the results derived from the method of least squares, in which the deflections at every station, without partiality, are fully taken into account and their most probable values exhibited.

Southampton, February 9, 1866.

XXIX. *On the Expansion of Saturated Vapours.* By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS.L. & E., &c.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the *Comptes Rendus* of the French Academy of Sciences for the 2nd of January, 1866, there appears a very interesting and important communication by M. A. Cazin, in which the author states the results of experiments which he has made for the purpose of testing one of the conclusions deduced from the laws of thermodynamics—viz., that when a vapour in the state of dry saturation performs work by expansion in a non-conducting cylinder, there is a certain critical temperature for each vapour such that below that temperature the expansion is accompanied by partial liquefaction, and above it by superheating.

M. Cazin mentions Professor Clausius and myself as having simultaneously predicted in 1850 the liquefaction of vapours under such circumstances; and M. G. A. Hirn as having shown, in 1862, that for certain vapours at certain temperatures, and in particular for ether, expansive working is accompanied by superheating. He then refers to M. Dupré as having proved, in 1864, that for each vapour there is a temperature below which expansive working causes liquefaction, and above it superheating, as above described.

Without detracting from the claim on the part of M. Dupré to having arrived at that result by an independent investigation, I wish to point out that the fact of there being such a critical temperature for each fluid is clearly indicated in my paper "On the Mechanical Action of Heat," published in the Transactions of the Royal Society of Edinburgh for 1850 (pages 170, 171), and that an approximate formula for that temperature, with its probable value for steam, is given by me in a paper "On Thermodynamics," published in the Philosophical Transactions for 1854 (pages 165, 166). The same formula with revised numerical data is given in 'A Manual of the Steam-engine and other Prime Movers,' page 386.

The passages bearing on this subject in the paper of 1850 are as follows (Edinb. Trans. vol. xx. pp. 170, 171). The italics are not in the original, being introduced now in order to point out the particular parts of those passages which are now specially referred to:—

"The apparent specific heat of a vapour at saturation is the quantity of heat which unity of weight of that vapour receives or gives out while its temperature is increased by one degree, its volume being at the same time compressed so as to bring it to the maximum pressure corresponding to the increased tem-

Phil. Mag. S. 4. Vol. 31. No. 208. March 1866.

P

perature. It is usually taken for granted that this quantity is the same with the variation for one degree of temperature of what is called the total heat of evaporation.

"... but I shall show that, according to the mechanical theory of heat, these two quantities are not only distinct, but in general of contrary signs."

(Then follows the investigation of a formula for the apparent specific heat at saturation of a vapour which in practice may be treated as perfectly gaseous, viz.

$$K_s = k \left\{ 1 + N \left(1 - \frac{\tau}{P} \cdot \frac{dP}{d\tau} \right) \right\},$$

in which k is the specific heat at constant volume, Nk the excess of the specific heat at constant pressure above the specific heat at constant volume, τ the absolute temperature, and P the pressure. After the formula comes the following passage):—

"For the vapours of which the properties are known, the negative terms of this expression exceed the positive at all ordinary temperatures"

In the paper of 1854, the expression corresponding to that in the preceding formula is $K_L - \frac{\alpha}{\tau}$, where K_L is the mechanical equivalent of the specific heat of the liquid from which the vapour comes, and $-\frac{\alpha}{\tau}$ an approximate value of $\tau \frac{d}{d\tau} \left(v \frac{dp}{d\tau} \right)$ (v being the excess of the *bulkiness* or volume of a unit of weight of the vapour above that of the liquid).

This second expression is applicable to all vapours whether perfectly gaseous or not; and by supposing the vapour to be perfectly gaseous, it becomes identical with the first expression. The value of τ at which the sign of the expression changes is of course $= \frac{\alpha}{K_L}$.

I may add that the corresponding formula of Professor Clausius, first published in 1850, indicates just as clearly that for each vapour there must be a temperature at which the sign of the specific heat, when the vapour is maintained at saturation, changes from negative to positive (*Abhandlungen über die mechanische Wärmetheorie*, page 38).

The originality and importance of M. Hirn's discovery, that for some fluids the temperature in question lies within the limits of temperatures attainable in ordinary experiments, are of course not affected by the preceding statement.

I am, Gentlemen,

Your most obedient Servant,

Glasgow, February 12, 1866.

W. J. MACQUORN RANKINE.

XXX. *On Saturated Vapours.* By W. J. MACQUORN RANKINE,
C.E., LL.D., F.R.SS.L. & E. &c.*

AS this paper consists almost wholly of formulæ, calculations, and tables, it is not suited for being read to a meeting, and therefore the following short abstract of its contents is alone offered for the purpose of being read aloud.

In the Edinburgh Philosophical Journal for July 1849, the Author proposed the following formula for the pressure of saturated vapour corresponding to a given boiling-point,

$$\log p = A - \frac{B}{t} - \frac{C}{t^2};$$

where t is the *absolute temperature* reckoned from the *absolute zero*, and A , B , and C are three specific constants, to be determined from at least three experiments on each substance; and he showed that the results of that formula agreed better with experiment than those of any other formula containing three constants only. In a series of papers on the Mechanical Action of Heat, read to the Royal Society of Edinburgh in 1850 and subsequent years, and in other publications also, the same formula is explained, and its use exemplified in various ways.

The first division of the present paper gives the results of the computation of the values of the constants A , B , and C for several fluids for which they had not previously been computed, the data being taken from the second volume of M. Regnault's *Relation des Expériences*, &c., published in 1862; and it is also shown that by the formula a conclusion was anticipated which M. Regnault has deduced from his experiments, viz. that "*the elastic force of a vapour does not increase indefinitely with the temperature, but converges towards a limit which it cannot exceed*" (*Relation des Expériences*, vol. ii. p. 647).

The second division of the paper is occupied chiefly with a comparison between the actual values of the pressures of saturation of the vapours of various fluids, and the values which those pressures would have if the vapours were perfectly gaseous.

In the first of the papers already referred to, read to the Royal Society of Edinburgh, and published in their 'Transactions' in 1850, the author proved, from the principles of thermodynamics, that the "*total heat*" of evaporation of a perfectly gaseous vapour must be represented in dynamical units by the expression

$$Jb + Jc't\ddagger,$$

* Abstract of a paper read to the Royal Society of Edinburgh in April 1865. Communicated by the Author.

† Jb in this expression corresponds to α in the preceding paper.

where b is a constant to be found by experiment, c' the specific heat of the vapour at constant pressure, and J the dynamical equivalent of a unit of heat, t being the absolute temperature, as before. In a paper read to the Royal Society of Edinburgh in 1855, but not published, the same formula was shown to express in dynamical units the *total heat of gasefication* of any substance under any constant pressure when the final absolute temperature is t . In the present paper the author equates that expression to another expression for the total heat of evaporation from the absolute zero, at a given absolute temperature t , as follows,

$$Jb + Jc't = J \int_0^t c'' dt + t \frac{dp}{dt} (v - v'');$$

in which v and v'' are the volumes of unity of weight of the substance in the gaseous and liquid states respectively, under the pressure p and at the absolute temperature t . Then putting for v its value in the perfectly gaseous state, namely

$$v = \frac{J(c' - c)t}{p},$$

where c is the specific heat of the gas at constant volume, and neglecting v'' as very small in comparison with v , there is found by integration the following value of the hyperbolic logarithm of the pressure of saturation (a being a constant to be deduced from one experiment for each fluid),

$$\text{hyp log } p = a - \frac{b}{(c' - c)t} + \frac{c'}{c' - c} \cdot \text{hyp log } t - \frac{1}{c' - c} \cdot \int_0^t \frac{dt}{t^2} \int_0^t c'' dt. \quad (\text{A})$$

When c'' is constant (as is approximately the case in some instances), the preceding equation becomes

$$\text{hyp log } p = a - \frac{b}{(c' - c)t} - \frac{c'' - c'}{c' - c} \text{hyp log } t. \quad (\text{B})$$

The pressures of various vapours as calculated, on the supposition of their being perfectly gaseous, by means of the preceding equations, are compared with their actual pressures—the general result being that when the vapours are rare the differences are small, and that when the densities increase the differences increase. For example, in the case of steam, the pressures calculated by equation (B) agree very closely with the actual pressures from 0° to 160° Centigrade; but above the latter temperature the difference gradually becomes considerable, and at 220° C. is about one-fiftieth part of the whole pressure. At 0° C. one pound of saturated steam occupies about 3400 cubic feet; at 160° C. about 5 cubic feet; and at 220° C. about 1.4 cubic foot.

The author also makes some comparisons between the actual

volumes of saturated vapours at given boiling-points, and the calculated volumes which they would fill if they were perfectly gaseous—and also between the actual latent heat of evaporation and the calculated latent heat of perfect gasefication. The general results are in accordance with what is already known, viz. that the actual volumes of vapours are less than those corresponding to the perfectly gaseous state, and the actual latent heat of evaporation less than the latent heat of gasefication; and the author further points out that the differences, in the case of steam, increase nearly as the absolute temperature.

XXXI. *On Secular Local Changes in the Sea-level.*

By D. D. HEATH, M.A., F.G.S.*

EMINENT geologists incline to the belief that at the “glacial epoch” a huge and more or less continuous capping of ice covered the whole arctic and subarctic region. And it is conceived that such a phenomenon may occur whenever, during a period of extreme excentricity of our orbit, the northern winter is nearly coincident with the time when the earth is in aphelion. In such a case, it is argued, we shall have an extreme climate, with severe and somewhat protracted winters and hot but shortened summers in the north, and a very equable climate in the south; and this will produce an accumulation of permanent ice in the one region and its disappearance in the other.

And these considerations have suggested to Mr. Croll in England, as well as to M. Adhémar in France, the thought that this gathering up of solid matter at one extremity of the earth would heap up the waters also of the ocean in high northern latitudes, and so account for the fact that over large areas in those latitudes the sea has left its mark on mountain-sides 1000, 2000, or more feet higher than its present relative level.

The general theory of changes of sea-level involved in this suggestion appeared to me well worth mathematical treatment, pursued to some rough numerical results—not only on account of the special facts of the glacial epoch, but because similar effects must follow upon any great change in the position of the solid parts of the earth, such as the rising of a continent. I should therefore have thought the following investigation worth completing and publishing, even though I had taken Mr. Croll’s letter to the ‘Reader’ of the 13th of January last, wherein he renounces the opinion that the theory will sufficiently account for the special facts, to be conclusive on that point.

But the truth is (as was pointed out by Mr. Fisher in the

* Communicated by the Author.

'Reader' so long ago as October 7), that the principle on which Mr. Croll attempts to solve the problem he has proposed is altogether erroneous.

His proposition is (Reader, September 2), "the surface of the ocean always adjusts itself in relation to the earth's centre of gravity, no matter what the form of the solid mass of the earth may happen to be;" the fallacy of which may be illustrated to the non-mathematical geologist by observing that the common explanation of the phenomena of the tides, by reference to the tendency of the ocean, under the influence of the moon or sun, to assume an elliptical shape, with the long axis pointing towards the luminary and the solid nucleus of the earth at the centre, would not be in anywise affected by supposing either luminary to be attached to the "solid mass of the earth" by a rod of insensible weight; whereas Mr. Croll's axiom would lead, in the first case, to the gathering up of the waters, not into two opposite semidiurnal tides respectively under and antipodal to the moon, but into one globular mass under it, leaving the opposite hemisphere absolutely bare once a day; and in the other case, to the abstraction of all the water of our globe to gather round that point within the sun's mass, which is the common centre of gravity of the two bodies. The truth is, the water will gather, but unsymmetrically, round the centre of gravity of the more powerful mass—as will be seen.

In the following investigation, which closely follows Laplace (*Méc. Cél.* liv. 3. ch. 4), I have simplified the problem by starting with the ideal case of a solid spherical nucleus, either homogeneous or with its matter disposed in concentric homogeneous layers, everywhere covered by the sea. I then suppose the equilibrium disturbed by a capping of ice at the north pole, of uniform thickness, and with a circular boundary to the south, and that the action of each vertical elementary prism on the waters may be conceived to be concentrated in one point of its length, and that all these points lie at equal distances from the centre of the "earth"—by which word I everywhere mean the terraqueous globe exclusive of this superficial capping. With these assumptions, we may omit any reference to the effect of the rotation of the earth (with which indeed the conditions would be incompatible, unless with much deeper seas than the existing ones); for the difference of surface between the primitive and the disturbed state, which is all we are inquiring about, would be the same in either case. It is further necessary to assume the disturbance of the surface to be nowhere great enough to make the curvature differ much from that of a sphere passing through the point under consideration, and having its centre in the centre of figure of the earth.

It is to be observed that the whole mass of the sea may be $\frac{1}{1500}$ or $\frac{1}{2000}$ of that of the earth; and of course the mass of ice is much less. And the disturbance of the sea-level, and the displacement (if any) of the centre of gravity of the solid nucleus with reference to the centre of figure or centre of gravity of the earth, can at most be of the like order of magnitude. Whenever, then, the mass of the ice, or a portion of it, or the amount of disturbance of the surface above or below the mean level, enters as a factor into any term of our expressions, we may take the other factors approximately, if we keep to this order of small magnitudes.

When the system has assumed its position of equilibrium, it is clear that the sea will form a surface of revolution round the polar axis.

The ice, and each element of it, will be tending to draw the earth towards it, as if it were a solid body, by its attraction, and will be counteracting that tendency by its pressure at the points where the two masses are in contact. It will also, by virtue of the difference of the amounts of its attraction on the different fluid particles, tend to produce relative motion among them; and this tendency must be counteracted by the mutual pressures and attractions of the parts of the earth, as determined by the figure it assumes.

The earth being nearly spherical, the action and reaction of attraction between it and each element of ice will be approximately the same as if its mass were collected at its centre of gravity; or as if the accelerating force due to the action of the element of ice on a particle of the earth at the centre of gravity were exerted equally, and in the same direction, on every particle of the earth. The effect, therefore, of the pressure of the ice on the earth, which counteracts this force, may be represented by its reverse; *i. e.* by applying to each particle of the earth an accelerating force equal, parallel, and opposite to that exerted by each element of ice at the centre of gravity, or approximately, according to the observation made above, at the centre of figure. And this mode of representation will obviously introduce no new pressure into the system, nor alter any existing tendency to relative motion among its particles.

We have then to consider what shape and relative position the solid and fluid parts of the earth must assume in order to equilibrium between their mutual pressures and impressed forces, each particle being taken to be solicited by (1) the direct attraction of each element of the ice upon it; (2) a force equal and opposite to that which the element of ice exerts at the centre of figure; and (3) the attraction of the whole earth.

The well-known condition of equilibrium is, that at the sur-

face, X , Y , and Z being the accelerating forces on any particle, we have

$$Xdx + Ydy + Zdz (= dp) = 0,$$

or (which is equivalent) that $\int (Xdx + Ydy + Zdz)$ must be invariable for every point on the surface. Taking the condition in the latter form, we may, after ascertaining the terms of the integral due to each of the forces, leave out all terms which are in themselves independent of the position of the particle, and equate the sum of the variable terms to zero.

Let, then, a be the radius of a sphere of equal volume with the earth; ρ the density of the earth, that of water being unity; β the thickness of a shell of water equivalent in mass to the shell of ice; and let μ' be the cosine of the north polar distance of any elementary prism of ice, ω' its longitude, and s (a constant quantity) its distance from the centre of figure of the earth; and let μ , ω , and r be the corresponding data for a particle at the earth's surface; and let f be the distance between these two points, and let dm represent the mass of the prism.

Then, as is well known, the part of the integral

$$\int (Xdx + Ydy + Zdz)$$

due to the action of dm is $-dm \int \frac{df}{f^2} = \frac{dm}{f}$. And $\frac{1}{f}$ may be expanded into a converging series,

$$\frac{1}{s} \left\{ P_0 + P_1 \frac{r}{s} + P_2 \frac{r^2}{s^2} + \&c. \right\},$$

where P_n is a known function of μ , ω , μ' , and ω' , of which it is enough here to say that $P_n = Q'_n Q_n +$ terms of the form $K \cos k(\omega - \omega')$, the coefficient K being independent of the longitudes, Q_n being a well-known function of μ ,

$$\left(\frac{1}{2^n \times 1 \cdot 2 \dots n} \frac{d^n (\mu^2 - 1)^n}{d\mu^n} \right),$$

and Q'_n being the same function of μ'^* .

The variable part of $\frac{dm}{f}$ is therefore

$$\frac{dm \cdot r}{s^2} \left\{ P_1 + P_2 \frac{r}{s} + P_3 \frac{r^2}{s^2} + \&c. \right\}. \quad (1)$$

* The successive values of Q_2 , Q_3 , &c. may be derived from the two first ($Q_0 = 1$, $Q_1 = \mu$) by the simple formula $(n+1)Q_{n+1} = (2n+1)\mu Q_n - nQ_{n-1}$, (Bertrand, *Calcul Différentiel*, p. 358). In actual calculation from this formula, it is useful to observe that, having at a previous step determined $(n-1)Q_{n-1}$, and thence deduced Q_{n-1} , we have only to add these two numbers together to obtain nQ_{n-1} .

The second part of our integral, that arising from the subtraction of the force at the centre of figure, may be deduced from this by observing that if we suppose the mass dm removed indefinitely backwards on the same radius, and at the same time increased in magnitude so as to preserve its attraction on the centre $\left(\frac{dm}{s^2}\right)$ the same throughout, we shall approach indefinitely to the state in which every part of the earth is solicited by equal and parallel attractions of the required magnitude. But by this process all the terms of the series (1), after the first, vanish. The first term, therefore, is the quantity sought to be subtracted, and the first and second parts of our integral united, as due to dm , become

$$\frac{dm \cdot r^2}{s^3} \left\{ P_2 + P_3 \frac{r}{s} + P_4 \frac{r^2}{s^2} + \&c. \right\}. \quad . \quad . \quad . \quad (2)$$

To obtain what is due to the whole action of the ice, we must express dm in terms of μ' and ω' , and integrate from $\omega'=0$ to $\omega'=2\pi$, and from $\mu'=\mu_1$ to $\mu'=1$, μ_1 being the extreme value of μ' .

Now dm is the volume of an elementary prism of length β , and whose base is $s \sin \theta' d\omega' \times s d\theta'$; or, as $\sin \theta' d\theta' = -d\mu'$, it is $\beta s^2 d\mu' d\omega'$, if we take the integration in the direction in which μ' increases. Substituting this value for dm , and integrating first in ω' , all the terms in $(\omega - \omega')$ obviously vanish, and the others take 2π as a factor, so that we get

$$\frac{2\pi\beta r^2}{s} \int_{\mu_1}^1 d\mu' \left\{ Q'_2 Q_2 + Q'_3 Q_3 \frac{r}{s} + \&c. \right\}$$

Now*

$$Q'_n = \frac{1}{2n+1} \left\{ \frac{dQ'_{n+1}}{d\mu'} - \frac{dQ'_{n-1}}{d\mu'} \right\},$$

whence

$$\int Q'_n d\mu' = C + \frac{1}{2n+1} \{ Q'_{n+1} - Q'_{n-1} \};$$

and when $\mu'=1$, $Q'_{n+1}=Q'_{n-1}=1$. Therefore between the limits the integral is

$$\frac{1}{2n+1} \{ Q_{n-1}^{(1)} - Q_{n+1}^{(1)} \},$$

which we will call A_n ; and then the part of the integral

$$\int (Xdx + Ydy + Zdz)$$

* Bertrand, as above. Murphy also, in his 'Electricity,' p. 50, gives the equivalent integral.

due to the action of the ice-cap is

$$\frac{2\pi\beta r^2}{s}\{A_2Q_2 + A_3Q_3 \frac{r}{s} + \&c.\} \dots \dots \dots (A)$$

To find the part due to the attraction of the earth, we may conceive the solid nucleus as made up of the same volume of matter of density 1 (*i. e.* of water), and of another equal volume of the actual density diminished by 1. The former part unites with the sea to make up a homogeneous spheroid. Now, r being measured from the centre of gravity of this homogeneous spheroid, and a being the radius of a sphere of equal volume, we may assume $r = a(1+y) = a\{1 + \alpha_2Q_2 + \alpha_3Q_3 + \&c.\}$; and then, neglecting y^2 , the attraction of this spheroid is known* to yield towards our integral

$$\frac{4\pi a^3}{3r} + 4\pi a^2 \left\{ \frac{\alpha_2Q_2}{5} + \frac{\alpha_3Q_3}{7} + \&c. + \frac{\alpha_nQ_n}{2n+1} + \&c. \right\};$$

or, putting $\frac{1}{a}(1-y)$ for $\frac{1}{r}$, and leaving out the constant term, it is

$$- \frac{4\pi a^2}{3} \{ \alpha_2Q_2 + \alpha_3Q_3 + \&c. \} + 4\pi a^2 \left\{ \frac{\alpha_2Q_2}{5} + \frac{\alpha_3Q_3}{7} + \&c. \right\}. (B)$$

The rest of the mass of the earth will act as if collected at its centre of gravity. And if this were at any sensible distance δ from the centre of figure, we should have for this action (precisely as for dm , only expanding in powers of $\frac{\delta}{r}$ instead of $\frac{r}{\delta}$, so as to converge) an expression which, δ being along the polar axis, becomes

$$\frac{\text{mass}}{r} \left\{ 1 + Q_1 \frac{\delta}{r} + Q_2 \frac{\delta^2}{r^2} + \&c. \right\};$$

and when we come to put all the variable parts of our integral together and equate corresponding terms to zero, there will be no other term (while we confine ourselves to the first order of magnitude) involving Q_1 , except this one multiplied by δ . We must therefore have $\delta=0$; or the nucleus must remain central, as before the disturbance. And then writing $\frac{1}{a}(1-y)$ for $\frac{1}{r}$,

* I use both Laplace's formulæ here without giving the proof. His own is notoriously defective; and I do not remember to have seen the proof of the second formula (for the attraction *at the surface*) correctly given in print. It follows, however, when the function Q_n has been independently ascertained, from Laplace's mode of reasoning.

and leaving out the constant term, we get for the last part of our integral

$$-\frac{\text{mass}}{a} \{ \alpha_2 Q_2 + \alpha_3 Q_3 + \&c. \} . . . \quad (C)$$

And finally, adding (A), (B), and (C) together, observing that the coefficients of $\{ \alpha_2 Q_2 + \alpha_3 Q_3 + \&c. \}$ in (B) and in (C) together make up the whole mass of the earth $\left(\text{or } \frac{4\pi\rho a^3}{3} \right)$ divided by a , equating to zero, and transposing, we get

$$\left. \begin{aligned} & \frac{4\pi\rho a^2}{3} \{ \alpha_2 Q_2 + \alpha_3 Q_3 + \&c. + \alpha_n Q_n + \&c. \} \\ & - 4\pi a^2 \left\{ \frac{\alpha_2 Q_2}{5} + \frac{\alpha_3 Q_3}{7} + \&c. + \frac{\alpha_n Q_n}{2n+1} + \&c. \right\} \\ & = \frac{2\pi\beta r^2}{s} \left\{ A_2 Q_2 + A_3 Q_3 \frac{r}{s} + \&c. + A_n Q_n \frac{r^{n-2}}{s^{n-2}} + \&c. \right\}, \end{aligned} \right\} \quad (D)$$

whence we may successively determine α_2 , α_3 , &c., and obtain the amount of disturbance,

$$r-a=a \{ \alpha_2 Q_2 + \alpha_3 Q_3 + \&c. \}.$$

In the physical problem before us $\frac{r}{s}$ is so nearly equal to 1, that we must take many hundreds of terms before $\frac{r^n}{s^n}$ would sensibly differ from 1; and in the meanwhile, Q_n being never greater than 1 and converging slowly*, while A_n diminishes pretty rapidly, the terms would have become exceedingly small. We may therefore write 1 for $\frac{r}{s}$, and a for r ; and then we get for the general term,

$$a\alpha_n \left\{ \frac{\rho}{3} - \frac{1}{2n+1} \right\} = \frac{\beta}{2} A_n.$$

I have had calculated for me the first fifty values of A_n for a cap of ice extending to N.P.D. 30° , and the values of Q_n and of the product $A_n Q_n$ corresponding to the disturbance at N.P.D. 35° , or about that of the north of England. For the divisor $\frac{\rho}{3} - \frac{1}{2n+1}$ I have then taken $\rho=6$, which I presume to be under the probable value, so as to make the calculation of the disturbance if anything too large; and, with the same object, I have used

* See Murphy, as above.

the ultimate limit $\frac{\rho}{3}$, or 2, for this divisor as soon as I reached the first set of negative terms in the series. With these data I give the approximate results for the disturbance: 1st, at the north pole, where $Q_n=1$; 2ndly, at the south pole, where $Q_n=\pm 1$ alternately; 3rdly, at N.P.D. 35° ; and 4thly, at the corresponding southern latitude, at which the values of Q_n are also the same in magnitude as in the north, but with the alternate signs changed.

1. At the north pole, summing together each set of positive and negative terms, we have the elevation

$$\frac{\beta}{2} \{ \cdot 13132 + \cdot 00623 + \cdot 00176 + \cdot 00101 + \cdot 00036 \},$$

or $\beta \times \cdot 07034$. If we suppose such a mass as two miles thickness of ice, or say 10,000 feet of the density of water, this would give an elevation over the mean level of some 700 feet. But it is to be borne in mind that, to obtain such a mass, we must rob the existing sea of the greater part of it. In our ideal case of an ocean covering the whole surface, each mile of ice taken from it would lower the mean level by about $\frac{133}{2000}$ ths, or 332 feet.

2. At the south pole we have also a relative elevation, but the amount is only

$$\frac{\beta}{2} \{ \cdot 03128 + \cdot 00128 + \cdot 00045 + \cdot 00042 + \cdot 00035 \},$$

or $\beta \times \cdot 01689$, less than one-fourth of that at the north pole.

3. At north polar distance 35° , as might be expected, the series is much less regular. Grouping positive and negative terms separately, it gives

$$\begin{aligned} \frac{\beta}{2} \{ & \cdot 03719 - \cdot 01485 + \cdot 00112 - \cdot 00559 + \cdot 00003 - \cdot 00456 \\ & + \cdot 00001 - \cdot 00079 + \cdot 00008 - \cdot 00035 + \cdot 00019 - \cdot 00012 \\ & + \cdot 00024 - \cdot 00004 + \cdot 00027 \}, \text{ or } \beta \times \cdot 00642. \end{aligned}$$

It will be seen that the positive terms by themselves descend and then rise again, and the negative terms rise and then fall by themselves; but it is certain that the *maxima* in each group will grow less and less; and though a hundred terms would have been more satisfactory than fifty, we may feel very confident that the elevation over the mean level for two miles of ice will not exceed 80 feet, which is really a considerable depression from the existing level.

4. At south polar distance 35° the series has for its principal

group of terms

$$\frac{\beta}{2} \{ \cdot 03046 - \cdot 00673 + \cdot 00145 - \cdot 00333 \},$$

after which the terms become so small and so nearly balanced as not to be worth setting down. The result for fifty terms is $\beta \times \cdot 01127$, or near upon as much again as the quantity obtained for northern elevation.

The general result, then, of the disturbance of the earth's surface is not such as was anticipated by those who first started this speculation, though it cannot surprise those who are familiar with the theory of the tides. The solid nucleus of our ideal earth remaining central, as we have seen, there is an accumulation of water towards the two opposite regions where the difference between the extraneous attraction on the surface and on the centre, which is the true disturbing force, is greatest. In the tidal theory, owing to the great distance of the luminary, these differences follow the same law, sensibly, under and opposite to the disturbing body; but in our case the changes are very rapid under and near the ice, which lies close to the surface, while they are gradual in the opposite hemisphere. Hence results an egg-shaped form of sea surface, with a comparatively sharp *little end* at the north, balanced by a *big-end* or broad-spread mass at the south, both ends combining to rob the equatorial region.

As I said at the beginning of this paper, the failure to account for the special facts of the glacial epoch does not deprive this theory of a certain positive value. If it cannot explain elevations or depressions of 1000 feet, it does teach us there is an agency at work in nature, which had perhaps been overlooked, which must be borne in mind in all speculations where tens of feet are material. The existing world, with its irregular continents and scooped-out seas, cannot have its water-surfaces level (regularly spherical or elliptical I mean); and with secular changes in the disposition of the solid parts, there must be local changes in the fluids, manifesting themselves in submersions or "raised beaches," which, when arising from this cause, must be in long and regular lines.

For a continent composed of matter between two and three times the density of water, and rising above its level, represents a sea over the same area with the addition of an attracting mass of more than the average depth and more than the density of this sea; and a hollow scooped out of a supposed level sea-bottom in like manner represents an abstraction of previously existing attractive force there. And these disturbances in the force will produce corresponding elevations or depressions of mean level

near the borders of the continent or hollow, and also at the antipodes. In such an irregular system as our globe actually is, these causes of disturbance are, no doubt, to a great extent antagonistic one to another; and he must have a wonderful sagacity, or be very rash, who should attempt to conjecture the total effect, at any place, of the existing configuration of land and sea. Still the fact remains, that the surface of the sea cannot be regular, and that the irregularities must shift and vary as the disposition of the land changes.

Kitlands, Dorking.

XXXII. *A Speculation concerning the relation between the Axial Rotation of the Earth, and the Resistance, Elasticity, and Weight of Solar Æther.* By Professor FREDERICK GUTHRIE*.

THE line of argument in the following paragraphs is this:—

If solar æther resists the translation of matter, and if solar space be filled with æther of uniform resisting power, then the effect of such an æther would be to tend to cause the earth to revolve on its axis in a direction opposite to that in which it actually revolves.

But if the æther has weight and elasticity, and if its resistance increases with its density, then solar æther might convert a portion of the orbital motion of the earth into axial rotation in the direction in which it actually revolves.

And hence it is not impossible that the axial rotation of the earth may be due to the conversion of a part of its orbital motion by the resistance of the solar æther, the solar æther having weight.

1. Gravitation explains both the translation of the earth and its retention in a circumsolar orbit. But the daily or axial rotation of the earth cannot be referred to the same force.

2. When a body moves in any path without rotation, that is, in such a manner that any fixed line in it retains its original direction, or any two positions of the same line are parallel to one another, then every point of the body traverses a path of the same length and shape.

3. If a circle moves without axial rotation in a circular orbit, all points on or in the circle describe circular orbits whose radii are all equal to the distance between the centre of the moving circle and the centre of the orbit.

4. If, on the moving circle, a point, which at starting is at a certain distance from the centre of the orbit, moves uniformly

* Communicated by the Author.

in a circle round the centre of the moving circle in the same direction as the moving circle moves in its orbit, and in such a manner as to move round the centre of the moving circle in the same time as the circle performs one orbital revolution, that point remains at the same distance from the centre of the orbit.

5. Inversely, when a circle moves without axial rotation in a circular orbit round a point, the points of greatest and least distance between the circle and the orbital centre travel round the circumference of the moving circle in such a manner that the line joining them, when produced, passes through the orbital centre. This line completes one revolution as the moving circle completes one revolution.

6. Hence the successive series of points of maximum distance* traverses an orbital path longer than the path described by the successive series of points of minimum distance by the circumference of a circle whose radius is equal to the diameter of the moving circle.

7. The same is true, *mutatis mutandis*, for any points in any body moving in any closed orbit.

8. Suppose that the earth at some time had orbital (by the action of gravity or otherwise) but no axial rotation.

9. If the space through which the earth travels in its motion round the sun be filled with a uniform medium of any degree of tenuity, which has any inertia or which in any way offers resistance to the passage of the earth through it, either frictional resistance to the earth's surface, or penetrative resistance towards its mass, then orbital motion must necessarily give rise to axial rotation.

10. Because the succession of points furthest from the sun will be continually subjected to the greatest resistance, and the points nearest to the sun to the least resistance during the earth's orbital motion; and because all other points of the earth's surface (if the resistance be only superficial) or of its mass (if it be penetrative) meet with greater or less opposition, according as they are further from or nearer to the sun.

11. Hence the resulting effect would be equivalent to a force upon the part of the earth furthest from the sun, tending to make the earth revolve on an axis perpendicular to its orbit-plane and in a direction opposite to the orbital rotation of the earth.

12. Thus if a sphere were to move (at first without rotation)

* The successive (with regard to the circumference of the wheel) series of points in which a rolling wheel touches the ground is *the* point (with regard to position referred to a vertical line through the centre of the wheel) where the wheel touches the ground.

between two parallel planes tangential to opposite ends of a diameter, and if the friction on one plane were greater than that on the other, the sphere would acquire a rotation in the direction it would have if rolling on the plane whose friction is greatest.

13. The actual orbital motion of the earth being elliptical (nearly circular), the part of the surface of the earth (for superficial resistance) whose resistance to the medium would tend to cause rotation in the same direction as the orbital motion of the earth, and the part whose resistance would tend to cause rotation in the opposite axial rotatory direction, are the two parts cut off by an elliptical (nearly circular) cylinder, one of whose focal axes passes through the sun, and whose plane radius vector reaches from the centre of the sun to the centre of the earth. For penetrative resistance, *mutatis mutandis*.

14. This principle may be easily illustrated experimentally in the following manner:—A heavy spherical ball is hung by a fibre of silk in a wide shallow cylindrical vessel of water. The water is made to gyrate slowly and steadily by the rotation in it of a little cardboard screw worked on the end of a stick by a thread wound round its axis. As soon as the water is in motion the ball begins to rotate on a vertical axis in the same direction. The orbital motion of the earth is here represented by the gyration of the water in the cylinder.

15. The actual axial rotation of the earth is in the same direction as its orbital rotation. Hence, if its axial rotation be due to the resistance of the æther, the resistance on the part of the earth nearest the sun must be greater than that on the remoter side.

16. This cannot be the case if the æther offers uniform resistance throughout, but it would result from the æther which lies nearer the sun being denser than the remoter æther.

17. Such a difference would result from the possession by the æther of weight and elasticity.

18. The idea of compressibility and elasticity is inseparable from that of æther; or rather the necessity for an æther has been felt in order to explain the phenomena of light and heat by means of travelling states of unequal compression and the recovery from such states—called undulation. Further, there can be no such thing as density, either constant or variable, and therefore no such thing as undulation without matter. Nor is it possible to conceive matter without weight.

19. Hence it is no extension of the ordinary conception of the æthereal medium to suppose that the æthereal medium extends throughout all space, indeed, but is more dense in the neighbourhood where other matter most abounds.

20. The hypothesis here proposed is therefore briefly this. The solar system is full of æther which has weight. It is massed most abundantly round the bodies of that system which have the most mass: chiefly round the sun. Spreading from the sun in all directions, it diminishes in density. In its passage round the sun the earth encounters the resistance of this æther. Although the side of the earth nearest to the sun performs the shorter path, yet it encounters æther of greater density than is met with by the remoter side. Consequently the earth revolves on its axis in the same direction as its orbital rotation.

21. The æther, to the undulations of which the phenomena of light and heat are usually attributed, is supposed to pervade space, both where matter is present and where it is not; it is supposed to be coextensive with space, and, though not necessarily even in a state of rest, of uniform density throughout. It is clear that the penetrative resistance offered by a medium of this kind to the passage of the earth through it, would effect an axial rotation in the earth in the same direction as that wrought by a merely superficial one such as friction. In the latter case, however, the resistance to a body's translation varies with the plane orbitolinear projection of its surface; in the former, with the volume of the body, possibly with its density, and so with its mass.

22. No attempt is made in the foregoing to account for the inclination of the axis of the earth to the plane of its orbit.

23. If inequality of æthereal resistance to different parts of the earth be the real and only cause of the earth's axial rotation, since the axial velocity of the earth is known, any one of the three following factors could be found if the other two were known.

(a) The age of the earth's orbit.

(b) Coefficient of resistance between æther and earth.

(c) Law of elasticity of æther.

24. The direction of the axis of a comet to its orbit has been regarded as evidence against the existence of a resisting æther medium, because it is supposed that such a medium would retard the nucleus less than the tail, on account of the smaller size of the former. This objection would be just, if the resistance were a purely superficial or frictional one. But if the æther pervades the substance of the comet, it is probable that the interpenetrative resistance between the comet and the æther may depend also upon the density of the matter of the comet; so that though the nucleus be smaller than the tail, yet, being denser, it may have about the same mass, and therefore suffer about the same resistance from the æther.

Royal College, Mauritius,
January 19, 1866.

XXXIII. *On an improved form of Statement of the New Rule for the Separation of the Roots of an Algebraical Equation, with a Postscript containing a New Theorem.* By Professor SYLVESTER, F.R.S.*

MY new rule (of which the demonstration will be found in a paper by the late lamented Mr. Purkiss in the last Number of the 'Cambridge, Oxford, and Dublin Mathematical Messenger') for separating the roots of an algebraical equation, I mean the rule which bears to Newton's rule generalized the same relation as Fourier's to Descartes's, is susceptible of a certain slight improvement as regards the mode of statement, which appears to me deserving of notice.

If we suppose $fx=0$ to be the equation in the theorem as originally stated, I have employed the double progression

$$\begin{array}{ccccccc} fx, & f_1x, & f_2x, & \dots & f_nx, \\ Gx, & G_1x, & G_2x, & \dots & G_nx, \end{array}$$

where $f_r x$ means $\left(\frac{d}{dx}\right)^r fx$, and $G_r x$ means $(f_r x)^2 - \gamma_r f_{r-1} x \cdot f_{r+1} x$, γ_r being a known function of r involving an arbitrary parameter, confined between limits of which one is dependent on n .

In applying the theorem, it becomes necessary to count the number of compound successions for which, on writing a given value a for x , $f_r \cdot f_{r+1}$ and $G_r \cdot G_{r+1}$ are both simultaneously positive, and also the number of the same for which $f_r \cdot f_{r+1}$, and $G_r \cdot G_{r+1}$ are simultaneously negative and positive respec-

tively, the succession

f_r	f_{r+1}
G_r	G_{r+1}

 in the first case constituting

what I have called a double permanence, and in the other case a variation-permanence. This latter is of course to be distinguished from a permanence-variation, which corresponds to the supposition of f_r, f_{r+1} bearing like, and G_r, G_{r+1} unlike signs—there being in fact four kinds of succession, viz. double permanences, variation-permanences, permanence-variations, and double variations.

If the enunciation of the theorem can be made to refer to double variations and double permanences exclusively, it is evident that something will have been gained in point of simplicity of statement†; and this can easily be effected in the manner following.

* Communicated by the Author.

† Moreover, so stated the theorem becomes more closely analogous to

Let $H_r x = (f_r x)^3 - \gamma_r \cdot f_{r-1} x f_r x \cdot f_{r+1} x$,
so that

$$H_r x = f_r x G_r x; \quad H_{r+1} x = f_{r+1} x \cdot G_{r+1} x;$$

then, when $f_r x, f_{r+1} x$ have the same sign, the nature of the succession $H_r x, H_{r+1} x$ will evidently be the same as that of $G_r x, G_{r+1} x$; but when $f_r x, f_{r+1} x$ have unlike signs, the nature of the succession $H_r x, H_{r+1} x$ will be contrary to that of $G_r x, G_{r+1} x$.

Accordingly when $\begin{array}{|c|c|} \hline f_r & f_{r+1} \\ \hline G_r & G_{r+1} \\ \hline \end{array}$ constitutes a double permanence,

$\begin{array}{|c|c|} \hline f_r & f_{r+1} \\ \hline H_r & H_{r+1} \\ \hline \end{array}$ will also constitute a double permanence; but

when $\begin{array}{|c|c|} \hline f_r & f_{r+1} \\ \hline G_r & G_{r+1} \\ \hline \end{array}$ constitutes a variation-permanence $\begin{array}{|c|c|} \hline f_r & f_{r+1} \\ \hline H_r & H_{r+1} \\ \hline \end{array}$

will constitute a variation-variation, *i. e.* a double variation.

If, then, we take for our double progression

$$\left\{ \begin{array}{l} f x, f_1 x, f_2 x, \dots f_n x, \\ H x, H_1 x, H_2 x, \dots H_n x, \end{array} \right\}$$

the rule, or rather the independent pair of rules referred to, will take the following simplified form.

Supposing a, b to be any two real quantities in ascending order of magnitude, on substituting for x first a and then b , in the simultaneous progressions above written, double permanences (in passing from a to b) may be gained, but cannot be lost: double variations may be lost, but cannot be gained. And the number of real roots included between a and b will either be equal or inferior to the number of double permanences so gained, and also equal or inferior to the number of double variations so lost—the difference, if there be any in either case, being some even

number. The value of γ_r is $\frac{\nu + r - 1}{\nu + r}$, where ν is limited not to fall within the limits 0 and $-n$. By ascertaining the gain of double permanences and the loss of double variations consequent on the replacement of a by b , we are furnished with two *independent* superior limits to the number of real roots included between a and b .

Fourier's. It may not be unreasonable to imagine that a third progression may remain to be invented such that the number of *triple* permanences and *triple* variations of sign in the three combined may afford a new superior limit, and so on *ad infinitum*; but this of course is at present a matter of pure conjecture.

Postscript.

It often happens that the pursuit of the beautiful and appropriate, or, as it may be otherwise expressed, the endeavour after the perfect, is rewarded with a new insight into the true. So it is in the present instance; for the substitution of the H for the G series, devised solely for the purpose of giving greater clearness to the enunciation of a known theorem, leads to a supplemental theorem which combines with and lends additional completeness and harmony to the original one.

At present the theory stands thus: a superior limit to the number of real roots between two limits a and b is afforded by counting, as x ascends from the one to the other, the loss of changes or gain of permanences (these two numbers are identical) in the f or Fourierian progression, and also by counting the loss of double changes, or gain of double permanences, in the f and H progressions combined: these two are distinct. We have thus the choice of three superior limits. I shall show that a fourth independent one is afforded by considering the loss of changes or gain of permanences in the single H progression, and combining it with such loss or gain in the single f progression.

We have

$$H_r x = f_r x \cdot G_r x,$$

where

$$G_r x = (f_r x)^2 - \gamma_r (f_{r-1} \cdot x)(f_{r+1} x),$$

γ_r being essentially positive for all values of γ .

It has been proved (see Mr. Purkiss's paper above referred to) that, when $G_r x = 0$,

$$G_r(x + \epsilon) = \frac{\epsilon}{\gamma_{r+1}} \cdot \frac{f_r x}{f_{r+1} x} G_{r+1} x,$$

ϵ being an infinitesimal.

Now suppose $H_r x = 0$. This may happen in two distinct ways, namely, either when $G_r x = 0$, or when $f_r x = 0$.

1. Let $G_r x = 0$, then

$$\frac{d}{dx} H_r x = f_r x \frac{d}{dx} G_r x.$$

Hence

$$\begin{aligned} H_r(x + \epsilon) &= \frac{\epsilon}{\gamma_{r+1}} \cdot \frac{(f_r x)^2}{f_{r+1} x} \cdot G_{r+1} x \\ &= \frac{\epsilon}{\gamma_{r+1}} \left(\frac{f_r x}{f_{r+1} x} \right)^2 H_{r+1} x. \end{aligned}$$

2. Let $f_r x = 0$, then

$$\frac{dH_{rx}}{dx} = G \cdot x \cdot f_{r+1}x;$$

also

$$G_{r-1}x = (f_{r-1}x)^2; \quad G_r x = -\gamma_r \cdot f_{r-1}x \cdot f_{r+1}x; \quad G_{r+1}x = (f_{r+1}x)^2.$$

Thus $H_{r-1}x$, $H_r(x+\epsilon)$, $H_{r+1}x$ are conformable in signs to

$$f_{r-1}, \quad -f_{r-1}x \cdot \epsilon, \quad f_{r+1}x \quad . \quad . \quad . \quad . \quad . \quad (\alpha)$$

in this case, and in the case preceding to

$$H_{r-1}x, \quad H_{r+1}x \in, \quad H_{r+1}x. \quad . \quad . \quad . \quad . \quad . \quad (\beta)$$

The above cases have reference to any *intermediate* H becoming zero; the final H is $(fx)^3$, and the last but one is

$$(f'x)^3 - \gamma_1(f''x \cdot f'x \cdot fx);$$

and accordingly when $fx=0$, H_x , $H(x+\epsilon)$ become of the same signs as

$$f'x; \epsilon f'x. (\gamma).$$

By combining the results (α) , (β) , (γ) , and denoting by ν the number of real roots included between (a) and (b) , it is easy to infer the equation

$$\nu = P - 2\phi + 2\eta,$$

where P is the number of permanences gained in passing up x from a to b in the H progression, ϕ is the collective number of times that any intermediate G vanishes at a moment when the preceding and subsequent H 's have like signs, and η is the collective number of times that any intermediate f vanishes at a moment when the two adjacent f 's have like signs. But if p is the number of permanences gained from the f (Fourier's series) by passing up x from a to b , we have $v = p - 2\eta$, where η represents the same quantity as above.

Hence

$$2\nu = P + p - 2\phi ;$$

and accordingly there emerges a new superior limit to ν , viz. $\frac{P+p}{2}$, an unlooked for and striking conclusion.

'Thus, *ex. gr.*, if $p = P + 2$, ν cannot be greater than $P + 1$, and therefore not greater than P , because it must differ from p or P (whose sum is necessarily even) by an even number*. To make the preceding demonstration absolutely rigorous, it would be necessary to consider the singular cases when several consecutive

* And so in general, when $p - \mathbf{P}$ is positive and not divisible by $4(p)$, the superior limit given by Fourier's theorem may be replaced by $\frac{p + \mathbf{P}}{2} - 1$.

terms of the H or G series vanish simultaneously, either with or without the corresponding terms of the f series vanishing too: this inquiry, which is necessarily tedious, and the result of which it is easy to anticipate, must be adjourned to a more suitable occasion.

If we call Λ the new superior limit, we have found,

$$\Lambda - \nu = \phi,$$

where ϕ is the collective number of values of x included between a and b for which any function $G_r x$ vanishes, whilst $H_{r-1}x$ and $H_{r+1}x$ have like signs; but since, when $G_r x = 0$, $f_{r-1}x$ and $f_{r+1}x$ must have like signs, ϕ may be defined more simply as the number of values of x between the given limits for which simultaneously, and for any value of r , $G_r x$ vanishes whilst $G_{r-1}x$ and $G_{r+1}x$ have like signs.

This quantity ϕ , the difference between the limit and the number of roots limited, may be odd or even, and not necessarily the latter, as is the case in all existing theorems of a similar nature.

Since $\Lambda = \frac{p+P}{2}$, it follows that when $P=0$, *i. e.* whenever the passage from a to b leaves the number of permanences in the H series unaltered, the limit p given by Fourier's theorem may be replaced by $\frac{p}{2}$ or $\frac{p}{2} - 1$, according as p is or is not divisible by 4.

XXXIV. *The Solution of a Problem in the Calculus of Variations by a New Method.* By PROFESSOR CHALLIS, M.A., F.R.S., F.R.A.S.*

THE problem to which the following investigation more immediately relates is the same as that discussed in the Numbers of the Philosophical Magazine for August 1861 and September 1862, viz. to determine the maximum solid of revolution, the surface of which is of given area and cuts the axis in two given points. In the second of these communications I have argued that there must be a particular solid which satisfies the proposed conditions, because a surface of revolution of given superficies might be such as to pass through the given points and at the same time enclose as small a volume as we please, or any volume that we please below a certain magnitude. Also I maintained that there is no valid reason for concluding that the Calculus of Variations fails to indicate this maximum, and that to effect the solution of the problem it is only required to discover the proper process of investigation. Having recently had

* Communicated by the Author.

occasion to review the process adopted in the above-mentioned communications, I first ascertained that it did not give, as I supposed, an absolute maximum, and subsequently that even where it agreed with the method commonly employed, it was essentially faulty. By admitting the principle of discontinuity (the legitimacy of which will presently come under consideration), and by reasoning in other respects according to the received rules of the Calculus of Variations, a solid of revolution was obtained such that the generating line of its surface is composed of the two ordinates drawn to the axis at the given points and a connecting curve. The analysis showed that the form of the curve is that described by the focus of an hyperbola rolling on a straight line, and it was proved that at its points of junction with the ordinates it must be continuous with them. As the rules of the Calculus of Variations indicated no other line capable of satisfying the data of the problem consistently with the condition of a maximum, it seemed to be a legitimate conclusion that this line gave an absolute maximum. If, however, the points be very near each other, and the given superficies be of great extent, it may easily be shown that the thin solid which the surface encloses under these circumstances would be much less than that enclosed by an equal amount of surface generated by the revolution of a segment of a circle the chord of which is the line joining the points; whence it necessarily follows that some fallacy is involved in the above conclusion. Having been convinced by these considerations that the problem remained unsolved, and not admitting that the Calculus of Variations could be at fault, I commenced a new investigation, and, after much ineffectual research, at length discovered a principle of solution which, I think, will be admitted to be legitimate and to give satisfactory results. This new solution I now proceed to develope.

Having made trial of an investigation conducted by polar coordinates r and θ , I obtained in the usual manner the differential equation

$$\frac{r \sin \theta (r + r'')}{(r^2 + r'^2)^{\frac{3}{2}}} + \frac{r' \cos \theta - 3r \sin \theta}{r(r^2 + r'^2)^{\frac{3}{2}}} = \frac{\sin \theta}{\lambda},$$

r' being put for $\frac{dr}{d\theta}$ and r'' for $\frac{d^2r}{d\theta^2}$. Now it appeared that this equation did not admit of a first integration till it was multiplied by the factor $r \cos \theta + r' \sin \theta$, after which an integral was obtained coinciding with that which results by employing rectangular coordinates and multiplying the equation by the factor $\frac{dy}{dx}$. In short, the two factors, and also the two processes, are

essentially the same, and the investigation by polar coordinates is here adverted to only because it first suggested to me the idea that the introduction of the factor has an important bearing on the solution of the problem. Since the differential equation from which the subsequent inferences are drawn is incomplete without the factor, and as the factor equated to zero satisfies the complete equation, this solution must certainly be taken as an answer, in part, to the proposed question. The integral of $r \cos \theta + r' \sin \theta = 0$, or $\frac{dy}{dx} = 0$, is $r \sin \theta = \text{const.}$, or $y = \text{const.}$; and, so far as this integration indicates, the required surface is *cylindrical*.

We have next to inquire what inferences may be drawn from the integral of the complete equation. The conditions of the problem being expressed by

$$\delta \int (y^2 + \lambda \sqrt{1+p^2}) dx = 0,$$

the usual process gives

$$2y dx + \lambda dx \sqrt{1+p^2} - d \cdot \frac{\lambda y p}{\sqrt{1+p^2}} = 0,$$

or

$$2y + \frac{\lambda}{\sqrt{1+p^2}} - \frac{\lambda y q}{(1+p^2)^{\frac{3}{2}}} = 0,$$

p being put for $\frac{dy}{dx}$ and q for $\frac{dp}{dx}$. After multiplying this equation by p and integrating, the result is

$$y^2 + \frac{\lambda y}{\sqrt{1+p^2}} = C,$$

C being the arbitrary constant introduced by the integration. We may now suppose that one of the points through which the surface has to pass is the origin of coordinates, in which case the above equation is satisfied if $y = 0$. Thus on this supposition $C = 0$, and we have

$$y \left(y + \frac{\lambda}{\sqrt{1+p^2}} \right) = 0.$$

Removing the factor y , the effect of which has just been taken into account, and attaching to the radical the double sign, as the theory of equations requires, we obtain the two equations

$$y + \frac{\lambda}{\sqrt{1+p^2}} = 0, \quad y - \frac{\lambda}{\sqrt{1+p^2}} = 0.$$

Each of these equations gives by integration an equation of the form

$$(x+c)^2 + y^2 = \lambda^2;$$

and it is particularly to be observed that the arbitrary constant c may be different for the two equations. Thus we have been led by rigid deduction to the equations of *three* lines,—one a straight line parallel to, and at an arbitrary distance from, the axis of x , which is the axis of revolution; and the two others circles of radius λ , having their centres at arbitrary positions on that axis.

Before proceeding to the next step of the reasoning, it will be proper to direct attention to an analytical principle which I have discussed in the September Number of 1862. It is there remarked that the processes of the calculus of variations generally, answer a proposed question by furnishing a differential equation between the variables, and that such equation requires to be afterwards treated according to appropriate rules. Now it is known from the theory of differential equations, that generally when the degree of the equation is higher than the first, the process of integration conducts to more than one solution. When the differential equation furnished by the calculus of variations is of this class, there appears to be no other legitimate course than that of employing the several solutions *conjointly* in order to satisfy the conditions of the problem, supposing no criterion to exist by which certain of the solutions might be shown to be inapplicable. In the paper just referred to, an instance is given of such reasoning by the solution of the problem of the brachistochronous course of a ship from one given position to another, the analysis conducting to the equations of two straight lines passing through the positions, and inclined at supplementary angles to the straight line joining them. The ship's course is accordingly partly on one of the straight lines, and partly on the other.

The same principle being applied in the problem before us, we have to satisfy the condition of drawing from one of the given points to the other a composite line, consisting of parts of two equal circles having their centres on the straight line joining the points, and of a straight line parallel to this line. Clearly this may be done by making the circles pass each through a point, with their concavities turned towards each other, and connecting them by the straight line. But our argument would still fail unless the analysis indicated the mode of junction of the straight line with the circles. To determine this, recourse must be had to the part of the variation freed from the sign of integration; which, if the integral be taken from $y=y_0$ to $y=y_1$, gives, by being equated to zero, the following equation:

$$\frac{\lambda p_1 y_1 \delta y_1}{\sqrt{1+p_1^2}} - \frac{\lambda p_0 y_0 \delta y_0}{\sqrt{1+p_0^2}} = 0.$$

Supposing the integral to be taken from one extremity of the straight line to the other, $y_1 \delta y_1$ and $y_0 \delta y_0$ are each indeterminate, and the equation is satisfied because $p = 0$ and $p_0 = 0$. If taken between either of the given points and the extremity of the corresponding circular arc, and if y_0 and y_1 be the respective ordinates at these limits, the second term of the above equation vanishes because $y_0 \delta y_0 = 0$; but since $y_1 \delta y_1$ is indeterminate, the other term can vanish only because $p_1 = 0$. We may hence infer that the straight line and the arcs are continuous at the points of junction, and that each arc is a quadrant of a circle. The generating line of the required surface of revolution is thus completely defined.

The result to which the foregoing investigation has conducted may now be stated in these terms:—*The solid consisting of a cylinder and two hemispherical ends of the same radius, is larger than any other solid of revolution having the same amount of surface and the same length of axis.*

If r be the common radius of the cylinder and the hemispheres, and $2c$ the given distance between the points, the surface of the solid is $4\pi cr$, and its content is $2\pi r^2 \left(c - \frac{r}{3} \right)$. Hence if h^2 be the given surface, the radius r is equal to $\frac{h^2}{4\pi c}$, and the content of the maximum solid of revolution is

$$\frac{h^4}{8\pi c} \left(1 - \frac{h^2}{12\pi c^2} \right).$$

It may be stated, as confirming the truth of the theorem, that on comparing the volume of a prolate spheroid of small eccentricity with that of a solid of the form above determined, having the same superficial area and the same axis, the latter was found to be the greater. A like result was obtained by comparison with a prolate spheroid of eccentricity nearly equal to unity. Supposing k to be the ratio of h^2 to $4\pi c^2$, the calculation for the case of small eccentricity gave the result that the prolate spheroid is less than the other solid if $(1-k)^3$ be positive. Hence k must be less than unity, or h^2 less than $4\pi c^2$. In the limiting case, $h^2 = 4\pi c^2$, the solid is plainly a sphere, the cylindrical part vanishing. If h^2 be greater than $4\pi c^2$, there is nothing corresponding to the cylindrical part, and the foregoing investigation is no longer applicable. But since, for the reasons adduced at the beginning of this communication, there must still be a maximum solid, it is necessary, in order to complete the solution of the problem, to enter upon a separate investigation for this case, which I now proceed to do.

In the first place, it may be asserted, in consequence of the previous reasoning, that the equation

$$2ydx + \lambda dx \sqrt{1+p^2} - d \cdot \frac{\lambda yp}{\sqrt{1+p^2}} = 0$$

must not now be integrated by means of a factor. It does not appear to admit of exact integration by any other mode of treatment; but obviously it is allowable to integrate it under its present form, and thus obtain an expression for the area of the curve. After changing, for convenience, the sign of the arbitrary constant λ , the integration gives

$$2 \int y dx = \lambda s - \frac{\lambda yp}{\sqrt{1+p^2}} + C, \quad . \quad . \quad . \quad (A)$$

s being the length of an arc of the curve reckoned from an arbitrary origin. It is next required to discover from this equation the nature of the curve to which the integral $\int y dx$ belongs. The following reasoning employed for this purpose is of a novel character, but I think that, upon consideration, every step of it will be found to be both legitimate and necessary. First, it may be remarked that the kind of surface which can alone satisfy the conditions of the problem must be such that its generating curve has *two* ordinates to the same abscissa. This will be at once apparent by conceiving the distance between the given points to be very small, and the given surface to be large. Next, as the equation (A) cannot apply to one of the ordinates rather than the other, it must embrace both. The necessity for a consideration of this kind is recognized by M. Lindelöf in his *Leçons de Calcul de Variations*, p. 224. Again, as the right-hand side of the equation contains the arc s , the integration necessarily proceeds continuously from one extremity of the arc to the other. Now all these conditions are fulfilled by integrating relatively to the ordinates of successive points of the curve from one limiting ordinate to another having the *same* abscissa, the common abscissa being any that we please. Accordingly the result of the integration may be put under this form,

$$\int y dx = \frac{\lambda}{2} (s_2 - s_1) - \frac{\lambda}{2} \left(\frac{y_2 p_2}{\sqrt{1+p_2^2}} - \frac{y_1 p_1}{\sqrt{1+p_1^2}} \right),$$

the right-hand side of the equation being a function of the arbitrary abscissa. But from the theory of the quadrature of curves it is known that by this operation, regard being had to the signs of the areas, we obtain a *segmental* area cut off by a straight line coincident in direction with the limiting ordinates. The question now reduces itself to finding a curve the segmental area of

which so cut off is expressed by the right-hand side of this equation. It is not difficult to see that this condition is fulfilled by a circle of radius λ , having its centre in an arbitrary position. Let α and β be the coordinates of the centre. Then for any abscissa x_1 , we have the two values $\beta \pm \sqrt{\lambda^2 - (\alpha - x_1)^2}$ of the ordinates, and the segmental area in question is the integral $\int y dx$ taken from $s=s_1$ to $s=s_2$, and from $y=\beta - \sqrt{\lambda^2 - (\alpha - x_1)^2}$ to $y=\beta + \sqrt{\lambda^2 - (\alpha - x_1)^2}$. Conceive C to be the centre of the circle, P and P' the points of intersection of the circle by the ordinates, and PAP' the intercepted arc. Then if ϕ be the angle which the tangent at P makes with the axis of x , we have

$$\frac{\lambda p_1}{\sqrt{1+p_1^2}} = \lambda \sin \phi. \quad \text{So for the point P',}$$

$$\frac{\lambda p_2}{\sqrt{1+p_2^2}} = \lambda \sin (\pi - \phi) = \lambda \sin \phi.$$

Also, since the angle CPP' is equal to ϕ , $\lambda \sin \phi = \alpha - x_1$ for both points. Hence the above equation becomes

$$\begin{aligned} \int y dx &= \frac{\lambda}{2} (s_2 - s_1) - \frac{1}{2} (\alpha - x_1) (y_2 - y_1) \\ &= \text{the sector CPAP'} - \text{the triangle CPP'} \\ &= \text{the segment PAP'}. \end{aligned}$$

From this result it may be inferred that the form of the curve which satisfies the condition of a maximum is that of the *circle*, and that the position of the centre is at disposal for fulfilling required conditions.

If it be objected that, although the investigation for the case in which h^2 is less than $4\pi c^2$ led to a circle having two ordinates to each abscissa, it was not necessary to take both into account by entering into such considerations as the foregoing, the answer is, that in that case the ordinates were *equal* with opposite signs, and that the result would consequently have been the same if the negative ordinates had been included. In fact, for this particular case and no other, the equation (A) is satisfied, if the curve be a circle, when only one set of values of y is taken into account in the integration, as may be thus shown. Integrating from $s=0$ to $s=s_1$, and from $y=0$ to $y=y_1$, the result is

$$\int y dx = \frac{\lambda s_1}{2} - \frac{(\alpha - x_1) y_1}{2},$$

and the integral is consequently the positive half of the segmental area, the chord of which is equal to $2y_1$; which plainly should be the case, the negative values of y having been excluded. Further, it may be remarked that, if the determination of the

curve for the case of h^2 being greater than $4\pi c^2$ had been made by the usual method of integrating a differential equation, it does not appear how the same analysis could have embraced the two cases of h^2 being less and greater than $4\pi c^2$. As it is, the form of the curve for the latter case has been ascertained from an expression for the area as a function of the abscissa, instead of an expression for the ordinate as a function of the same variable.

From the result of the investigation for the case in which h^2 is greater than $4\pi c^2$, it may be concluded that, if the surface of the maximum solid of revolution be required to pass through two points of the axis distant from each other by $2c$, the surface is that generated by the segment of a circle, the radius of which may be calculated from the given value $2c$ of the chord and the given superficial area h^2 of the solid.

Since the coordinates of the centre of the circle are at disposal, the generating arc may terminate at any two given points not situated on the axis of revolution, and the surface generated by the arc and the coordinates of the points will enclose a maximum solid if the curved part of the surface be given.

Also, if the two points be equidistant from the axis and be supposed to approach indefinitely near to each other, the generating line becomes a complete circle, and the solid is a *ring* having a circular transverse section. Hence from the foregoing reasoning it may be concluded that this ring is larger than any other having the same superficies, and the same radius either interior or exterior, but a different form of transverse section.

Let us now suppose that the surface generated by the extreme ordinates and the connecting arc is given, and that the solid enclosed by this composite surface is required to be a maximum. In this case, if r and r' be distances of points of the plane circular areas from the axis, we shall have the additional terms

$$\delta \int \lambda r dr + \delta \int r' dr',$$

to be integrated from $r=0$ to $r=y_1$, and from $r'=0$ to $r'=y_2$. Thus the total quantity freed from the sign of integration will be

$$\left(\frac{p_2}{\sqrt{1+p_2^2}} + 1 \right) \lambda y_2 \delta y_2 - \left(\frac{p_1}{\sqrt{1+p_1^2}} - 1 \right) \lambda y_1 \delta y_1,$$

and the values of y_1 and y_2 have to be determined. Hence neither δy_1 nor δy_2 may be equated to zero, and we shall therefore have

$$\frac{p_2}{\sqrt{1+p_2^2}} + 1 = 0, \quad \frac{p_1}{\sqrt{1+p_1^2}} - 1 = 0;$$

or $p_2 = -\infty$ and $p_1 = +\infty$. This proves that the extreme ordi-

nates are continuous with the curve, and that the curve is a *semi-circle*, the radius of which is c . Hence, h^2 and c being given, the length of the ordinates and the content of the solid may be calculated.

The method of solution adopted in the August Number of 1861, gave for the connecting line in the last example a portion of the curve described by the focus of an hyperbola rolling on a straight line. The foregoing argument has shown that the fallacy of this determination consists in making use of an integral obtained by means of a factor, and not subsequently taking into account what was indicated by the factor.

Although it results from this new method that the curve just mentioned has no application in the solution of the problem that has been under discussion, I think it worth while to point out that the length and area of that curve are erroneously calculated in the August Number, and to obtain the correct values, as the process will serve to illustrate a part of the reasoning in the present communication. The semiaxes of the hyperbola being a and b , the differential equation of the curve is

$$1 + p^2 = \left(\frac{2ay}{b^2 - y^2} \right)^2,$$

which remains the same whether y be positive or negative. But for the present purpose it will suffice to consider only the positive values of y . By supposing that $p^2 = 0$, and substituting $a^2e^2 - a^2$ for b^2 , we obtain $a(e-1)$ for the positive minimum value of y , and $a(e+1)$ for the positive maximum value. Also the curve is evidently an *oval* symmetrically disposed about the maximum ordinate, and the extreme abscissæ are those corresponding to the ordinates $y=b$. For determining the length of the curve, we have

$$ds = \pm \frac{2ay \, dy}{\sqrt{4a^2y^2 - (b^2 - y^2)^2}},$$

the $+$ sign applying from the minimum to the maximum ordinate, and the $-$ sign from the maximum to the minimum ordinate. Taking the positive sign and integrating,

$$\cos \left(\frac{s}{a} + k \right) = \frac{a^2e^2 + a^2 - y^2}{2a^2e}.$$

Hence, if the integral be taken from $y=a(e-1)$ to $y=a(e+1)$ and be doubled, the periphery of the oval is found to be $2\pi a$. By integrating from $y=b$ to $y=a(e+1)$, and from $y=a(e-1)$

to $y=b$, and doubling the results, there will be obtained for the length of the upper part of the curve lying between the extreme ordinates $2\pi a - 2a \cos^{-1} \frac{1}{e}$, and for the length of the lower part $2a \cos^{-1} \frac{1}{e}$.

The differential expression for calculating the area is

$$ydx = \frac{(b^2 - y^2)ydy}{\sqrt{4a^2y^2 - (b^2 - y^2)^2}},$$

from which by integration may be obtained

$$\int ydx = \frac{1}{2} \left(4a^2e^2 - (a^2 + a^2e^2 - y^2)^2 \right)^{\frac{1}{2}} + a^2 \sin^{-1} \frac{a^2 + a^2e^2 - y^2}{2a^2e} + C.$$

This integral, taken from $y=a(e-1)$ to $y=a(e+1)$, is πa^2 , which, according to what has been already said, is the area of the left-hand segment cut off by the maximum ordinate. Hence the whole area of the oval is $2\pi a^2$.

By integrating from $y=a(e-1)$ to $y=b$, and from $y=b$ to $y=a(e+1)$, the double areas will be found to be respectively

$$-\pi a^2 + 2ab + 2a^2 \sin^{-1} \frac{1}{e},$$

and

$$3\pi a^2 - 2ab - 2a^2 \sin^{-1} \frac{1}{e}.$$

The first of these is equal to the area bounded by the extreme ordinates, the included abscissa, and the lower part of the curve, and is *negative*, because dy and dx have different signs from $y=a(e-1)$ to $y=b$. The other area is that bounded by the extreme ordinates, the included abscissa, and the upper part of the curve, and is evidently positive. It is not possible from these results to calculate the areas of the two parts into which the oval is divided by the straight line joining the extremities of the limiting ordinates, because the length of this line can only be known by obtaining y as an explicit function of x .

Cambridge, February 20, 1866.

XXXV. *On some Properties of Soap-bubbles.*

By J. BROUGHTON, B.Sc., Chemical Assistant, Royal Institution.*

THE colours of a soap-bubble, proverbial for their beauty, have long furnished an apt illustration of the optical effects produced by thin films; but the fragility of the subject has probably prevented their closer examination by other means than that of the unaided eye. It is, however, well known that those parts of the bubble which at a short distance appear of one homogeneous tint reveal, on closer inspection, remarkable streaks and curves of various brilliant colours, which doubtless denote portions of the bubble where equal thicknesses of film produce the interference of the same set of rays. In a thin bubble it is rarely that a space exists of a millimetre square without many of these streaks being visible, and it is only by the preponderance of one particular tint that the effect of a uniformly coloured surface of considerable extent is produced. The oleate-of-soda and glycerine solution, invented by M. Plateau, renders the employment of soap-bubbles for many experimental purposes now comparatively easy; and the vexatious bursting of the bubble at the moment of observation is nearly entirely obviated by their remarkable persistency. Bubbles blown with this solution are admirably adapted for optical experiments.

A bubble thus formed and placed on a wire ring under a glass shade will, after standing for an hour, frequently exhibit at its upper pole a circular black spot one-third or even half an inch in diameter. The black is intense, but it always possesses the property of reflecting a small amount of light. In this position it is easy to examine it by means of a lens, which renders evident optical effects of great splendour and interest, and moreover reveals the film to be always in motion. These appearances suggested the employment of a compound microscope for their observation. For this purpose a strip of cork of a convenient size for the stage of the microscope was covered with black paper, and on this a small wire ring was placed, on which a bubble was blown by means of a caoutchouc tube of small bore. The bubble was strongly illuminated by a good condenser, so that the light after reflexion might pass through the microscope.

When thus viewed, the film exhibits optical phenomena of the utmost magnificence, which are difficult to describe. The appearances observed in and near the black spaces above referred to, were of especial splendour. On the black ground moved specks of brilliant yellow and orange, which again contained smaller spots of blue and black of almost every geometrical form, but

* Communicated by the Author.

always in rapid motion. Many other appearances were observed; among the most common are spots of such regularity that at first sight they produce the effect of structure. Under a high power, these latter are resolved into series of Newton's rings of excessive minuteness. The variety of the phenomena is quite remarkable; but the most commonly occurring effects are those in which the colours red and green prevail. The motion appears to be invariable. It is diminished, though not prevented, by first passing the light through a layer of water and covering the bubble by a small glass shade.

A flat film will also produce similar effects to a bubble when viewed through the microscope, but the latter was found to answer somewhat better.

The determination of the thickness of the film in which such effects are produced is a matter of some interest. Optical methods are practically inapplicable, since various thicknesses will produce the same colour, provided they fulfil the condition of being constantly either even or odd multiples of $\frac{\lambda}{4}$. It was, however, attempted to determine the mean thickness by the following method, and the results are probably approximately correct. It consists in determining the weight of the bubble in the following manner:—A mixture of hydrogen and air was prepared which, by filling a bubble, would cause it just to float in the atmosphere without showing any considerable tendency to ascend or descend. The bubble was carefully freed from an adhering drop, and was measured by making it (while still attached to the pipe) appear to coincide with the divisions of a measure when viewed at a considerable distance. Such a determination gave the following data:—

A mixture of 1 volume of hydrogen and 16 volumes of air caused a bubble of 90 millims. diameter to float.

Let r = radius of bubble = 45 millims;

c = weight of 1 cubic centimetre of atmospheric air
= 0.00129318 grm.;

h = density of hydrogen = 0.0691;

s = spec. grav. of bubble-solution = 1.1;

w = weight of bubble;

t = thickness of film;

then

$$\frac{4r^3\pi}{3} c - \frac{4r^3\pi}{3} \left(\frac{16+h}{17} \times c \right) = w*.$$

* So small is the ratio of the weight of a bubble to its volume, that they can be made to ascend by dexterously filling them with warm breath.

Phil. Mag. S. 4. Vol. 31. No. 208. March 1866. R

Also

$$4r^2\pi st = w \text{ very nearly.}$$

Equating these values of w and reducing,

$$t = \frac{cr(1-h)}{51s}.$$

Whence, substituting values,

$$t = .000965 \text{ millim., or } \frac{1}{28000} \text{th of an inch nearly.}$$

Other similar determinations gave values ranging from $\frac{1}{19,000}$ to $\frac{1}{35,000}$.

These liquid films are therefore surpassed in thinness by the thinnest gold-leaf, though it is probable that after standing some time the portions near the upper surface of the bubble actually exceed the latter in tenuity. The thickness of the black spot itself corresponds, in Newton's Table for water, to a thickness of $\frac{3}{8,000,000}$ inch. When it is remembered that such films reveal so much mobility and such brilliant chromatic effects under the microscope, and moreover owe their cohesion to but one-eightieth part of substance of such a complex atomic character as oleate of soda, another illustration is afforded respecting the divisibility of matter*.

XXXVI. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 82.]

Feb. 12, 1866. **D**R. DROSIER read a paper "On the Functions of the Air-cells, and the Mechanism of Respiration, in Birds."

After brief mention of the additions made to our knowledge of these matters by numerous distinguished physiologists, he remarked that still more remained to be done—a proof of the difficulty of the subject. Several of the commonly received views are quite untenable, —such as that the air-cells are intended to assist in supporting the bird in flight, by rendering it lighter, in consequence of the rarefaction of the air in the air-cells, and the hollow bones ; and again,

* In anticipation of probable inquiries, I append the recipe for the bubble-solution of M. Plateau, which can also be obtained of Mr. Ladd, Beak Street :—

Dissolve one part of *pure* oleate of soda in fifty parts of distilled water, and mix this solution with two-thirds of its volume of pure glycerine.

that the air-cells are a sort of second respiratory apparatus, so that birds may be described, as they were by Cuvier, as animals having a double respiration. In disproof of these views, it was shown that a pigeon weighing 10 ounces, or 4375 grains, would have its weight in air diminished by less than one grain in consequence of the rarefaction of the air in its air-sacs and hollow bones; so that the floating-power resulting from such rarefaction would be almost inappreciable. Again, the air-cells are bounded by delicate membranes, in which the blood-vessels are very minute and sparsely scattered. Hence very little blood is offered for oxidation in them.

Some of the earlier observers, as Harvey and Perrault, in the middle of the seventeenth century had correctly described the air-cells of birds as sacs that enclose and confine the air received from the openings, on the inferior surface of the lungs, in which the bronchi terminate. Later observers, however, have generally fallen into the error that the air passes from the air-sacs into the cavities of the peritoneum and the pericardium, and even extends itself between the muscles, and beneath the skin in some cases; and notwithstanding that Guillot and Sappey have shown that the air does not pass out of the air-sacs, such errors are repeated even at the present day. The lungs of birds are not very elastic, are fixed to the ribs at the upper part of the thorax by close cellular tissue, and bound down by an aponeurosis formed by the tendons of the pulmonary diaphragm; so that they cannot draw in much air by expansion. They are moreover small, and are penetrated by the principal bronchi, which open upon their surfaces. Such lungs are quite incapable of acting in inspiration in the same manner as the lungs of reptiles and mammals. Capacious membranous bags are therefore provided to receive the inspired air, the volume of which is much greater in the case of birds than in the case of mammals. But the larger quantity of air inspired would be of little use if it were merely drawn into the air-sacs to be simply expelled again; for the greater part of the inspired air does not pass through the lungs, but direct through certain large bronchial tubes into the air-sacs situated within the thorax. There are another set of air-sacs situated without the thorax—namely, two very large sacs in the abdomen, and several others anterior to the thorax. When the thoracic air-sacs expand, the others contract, and *vice versd*. The alternate expansion and contraction of the two sets of air-cells causes currents of air to play continually through the spongy tissue of the lungs peculiar to birds, and to pass between the almost naked capillaries, first described by Mr. Rainey (in 1848) as forming the only walls of the areolar spaces that answer to the air-cells of the mammalian lung. The air-spaces between the capillaries are, according to Mr. Rainey's measurements, only $\frac{1}{9600}$ th of an inch, and the quantity of air in them must soon be deprived of oxygen and saturated with carbonic acid. Hence the necessity of its continual change. This change is effected by constant streams of air that fan the capillaries in passing from one set of air-sacs to the other. The intricate courses which the air takes in passing in and

out of the air-cells and bronchial tubes of various orders is difficult to describe, especially without diagrams.

The respiration of birds, even when in repose, has been shown to be much more active than that of mammals. But in order that birds may be equal to the enormous exertion required of them for sustaining themselves in the air for considerable periods of time, very ample provision must be made for respiration. If therefore the lungs were constructed after the mammalian type, they would require to be very large, and powerful muscles must have been provided for the respiratory movements. But this would add unduly to the weight of the body. The lungs therefore are small, very porous, and light; yet nevertheless their efficiency is ensured by a more minute division of the capillaries, and a more complete exposure of these to the action of the air supplied so abundantly from the capacious air-sacs. In short, more perfect *localized* instruments of respiration cannot be conceived.

Our great physiologist, John Hunter, believed it impossible that the ribs and sternum of a bird could move while the powerful pectoral muscles are engaged in flight. He therefore thought that the air-sacs of birds might be intended to act as reservoirs of air to be used in respiration during flight. These sacs, however, do not hold enough air to support the respiration of a bird for two minutes; for in that time, if the trachea of a bird be tied, it dies; yet many birds continue on the wing for hours together. Sappey has endeavoured to explain the difficulty which occurred to Hunter by pointing out that the great pectoral muscles of birds arise exclusively from the sternum, and not at all from the ribs, as they do in mammals. But this explanation only removes a part of the difficulty; for the ribs are so articulated with the sternum, that they cannot move unless the sternum moves also. Now the sternum in respiration moves at its articulations with the two coracoid bones, these bones being fixed in regard to the sternum and humerus in the movements of flight. It might seem, therefore, that when the pectoral muscles contract, the sternum would be drawn powerfully upwards as the wings are drawn downwards, and so the sternum and ribs fixed. But this is not so; for the fibres of these muscles converge towards and pass over the coracoid bones on their way to be inserted into the ridge of the humerus, and they act in the direction of the axis of the coracoid; so that they only draw the sternum and coracoid together more closely, and do not tend to flex these bones on one another. The common inspiratory muscles are therefore free to act, whether the pectorals are in action or not. To be more exact, the line of action of the great pectoral muscle lies a little below the coracoid bone, and parallel to its axis. Hence, in contracting, the muscle will tend to depress the sternum, and so assist the inspiratory muscles, and render inspiration deeper in flight than when the wings are closed.

The author gave a mathematical as well as an experimental proof that the external intercostal muscles raise both the ribs to which they are attached, and that the internal intercostals depress both ribs.

A frame of wood, in the form of a parallelogram with hinges at the angles, represented two ribs, the spine, and the sternum. An india-rubber ring was passed over a peg in the upper rib and another in the lower rib, at different distances from the spine, to represent the intercostal muscle. Both ribs were elevated or depressed according as the upper peg was nearer to, or further from the spine than the peg in the lower rib.

The hollow bones are filled with air, not for respiratory purposes, but to remove the moisture from the interior of the bones secreted by the endosteum, which would otherwise accumulate and defeat one of the objects for which the bones are hollow, namely, to diminish their weight,—the other object being to increase their strength. The author proposes to publish his views in a separate form so soon as he shall have leisure to complete certain experimental investigations that he has devised.

ROYAL SOCIETY.

[Continued from p. 155.]

January 18, 1866.—Lieutenant-General Sabine, President, in the Chair.

The following communication was read :—

“On the Spectrum of Comet 1, 1866.” By William Huggins, F.R.S.

The successful application of prismatic analysis to the light of the nebulæ showed the great importance of subjecting the light of comets to a similar examination, especially as we possess no certain knowledge of the intimate nature of those singular and enigmatical bodies, or of the cosmical relations which they sustain to our system. The importance of a prismatic analysis of cometary light is enhanced by the consideration of the general resemblance which some of the nebulæ present to the nearly round vaporous masses of which some comets, in some positions at least in their orbits, appear to consist,—a resemblance which suggests the possible existence of a close relation between nebulous and cometary matter.

I made several unsuccessful attempts to obtain a prismatic observation of Comet 1, 1864. The position of the comet and the weather were unfavourable. M. Donati succeeded in making an examination of the spectrum of this comet. “It resembles,” says M. Donati, “the spectra of the metals; in fact the dark portions are broader than those which are more luminous, and we may say these spectra are composed of three bright lines”*.

Yesterday evening, January 9, 1866, I observed the spectrum of Comet 1, 1866. The telescope and spectrum-apparatus which I employed are described in my paper “On the Spectra of some of the Nebulæ” †.

* Monthly Notices, Royal Astronomical Society, vol. xxv. p. 114.

† Phil.Trans. 1861, p. 421.

The appearance of this comet in the telescope was that of an oval nebulous mass surrounding a very minute and not very bright nucleus. The length of the slit of the spectrum-apparatus was greater than the diameter of the telescopic image of the comet.

The appearance presented in the instrument when the centre of the comet was brought nearly upon the middle of the slit, was that of a broad continuous spectrum fading away gradually at both edges. These fainter parts of the spectrum corresponded to the more diffused marginal portions of the comet. Nearly in the middle of this broad and faint spectrum, and in a position in the spectrum about midway between *b* and *F* of the solar spectrum, a bright point was seen. The absence of breadth of this bright point in a direction at right angles to that of the dispersion showed that this monochromatic light was emitted from an object possessing no sensible magnitude in the telescope.

This observation gives to us the information that the light of the coma of this comet is different from that of the minute nucleus. The nucleus is self-luminous, and the matter of which it consists is in the state of ignited gas. As we cannot suppose the coma to consist of incandescent solid matter, the continuous spectrum of its light probably indicates that it shines by reflected solar light.

Since the spectrum of the light of the coma is unlike that which characterizes the light emitted by the nucleus, it is evident that the nucleus is not the source of the light by which the coma is rendered visible to us. It does not seem probable that matter in the state of extreme tenuity and diffusion in which we know the material of the comæ and tails of comets to be, could retain the degree of heat necessary for the incandescence of solid or liquid matter within them. We must conclude, therefore, that the coma of this comet reflects light received from without; and the only available foreign source of light is the sun*. If a very bright comet were to visit our system, it might be possible to observe whether the spectra of the coma and the tail contain the dark lines which distinguish solar light. If the continuous spectrum of the coma of Comet 1, 1866, be interpreted to indicate that it shines by reflecting solar light, then the prism gives no information of the state of the matter which forms the coma, whether it be solid, liquid, or gaseous. Terrestrial phenomena would suggest that the parts of a comet which are bright by reflecting the sun's light, are probably in the condition of fog or cloud.

We know, from observation, that the comæ and tails of comets are formed from the matter contained in the nucleus.

The usual order of the phenomena which attend the formation of a tail appears to be that, as the comet approaches the sun, material is thrown off, at intervals, from the nucleus in the direction towards

* This conclusion is in accordance with the results of observations on the polarization of the light of the tails of some comets. Some of these observations appear to have been made with the necessary care. See J. P. Bond's "Account of the Great Comet of 1858," *Annals of the Astronomical Observatory of Harvard College*, vol. iii. pp. 305-310.

the sun. This material is not at once driven into the tail, but usually forms in front of the nucleus a dense luminous cloud, into which for a time the bright matter of the nucleus continues to stream. In this way a succession of envelopes may be formed, the material of which afterwards is dissipated in a direction opposite to the sun, and forms the tail. Between these envelopes dark spaces are usually seen.

If the matter of the nucleus is capable of forming by condensation a cloud-like mass, there must be an intermediate state in which the matter ceases to be self-luminous, but yet retains its gaseous state, and reflects but little light. Such a non-luminous and transparent condition of the cometary matter may possibly be represented by some at least of the dark spaces which, in some comets, separate the cloud-like envelopes from the nucleus and from each other.

Several of the nebulae which I have examined give a spectrum of one line only, corresponding in refrangibility with the bright line of the nucleus of the comet referred to in this paper. Other nebulae give one and two fainter lines besides this bright line. Whether either or both of these were also present in the spectrum of this comet I was unable to determine. The light of the comet was feeble, and the presence of the continuous spectrum made the detection of these lines more difficult. I suspected the existence of the brighter of these lines. I employed different eyepieces, and also gave breadth to the bright point by the use of the cylindrical lens, but I was not able to obtain satisfactory evidence of more lines than the bright one already described.

In my paper "On the Spectra of the Nebulae," I showed that this bright line corresponds in refrangibility with the brightest of the lines of nitrogen. This line may perhaps be interpreted as an indication that cometary matter consists chiefly of nitrogen, or of a more elementary substance existing in nitrogen.

The great varieties of structure which may exist among comets, as well as the remarkable changes which the same comet undergoes at different epochs, will cause all those who are interested in the advance of our knowledge of the cosmical relations of these bodies, and of the gaseous nebulae, to wait with some impatience the visit of a comet of sufficient splendour to permit a satisfactory prismatic examination of the physical state of cometary matter during the various changes which are dependent upon the perihelion passage of the comet.

January 25.—Lieutenant-General Sabine, President, in the Chair.

The following communication was read:—

"Note on the Secular Change of Magnetic Dip, as recorded at the Kew Observatory." By Balfour Stewart, M.A., LL.D., F.R.S., Superintendent of the Observatory.

The President of this Society has already called the attention of the Fellows to the annual values of the magnetic inclination at

Toronto, as deduced from the monthly determinations. In doing so he remarked that "the general effect of the disturbances of the inclination at Toronto is to increase what would otherwise be the amount of that element; therefore, if the disturbances have a decennial period, the absolute values of the inclination (if observed with sufficient delicacy) ought to show in their annual means a corresponding decennial variation, of which the minimum should coincide with the year of minimum disturbance, and the maximum with the year of maximum disturbance." At Toronto, where the true secular change is very small, the effect of this superimposed variation is very visible, so that the yearly values of the inclination appear to increase up to the period of maximum disturbance and to decrease after it. At Kew the general effect of disturbances is probably the same as at Toronto—that is to say, tending to increase the inclination; but the secular change being considerable, and tending to decrease the inclination, the joint effect of the secular change and the superposed variation might be expected to appear in a diminution of the yearly secular change for those years during which the disturbances are increasing from their minimum to their maximum value, and in an increase of the yearly secular change for those years during which the disturbances are decreasing from their maximum to their minimum.

The Kew records appear to exhibit a variation of this nature. Observations of dip were commenced at the Kew Observatory in 1854; and by comparing a good number of observations taken during the latter months of 1854, with two circles and four needles, with observations taken with the same circles and needles during the same months of 1855, we obtain a yearly secular change of $2^{\circ}24'$.

During the years from 1856 to 1859 inclusive, monthly observations were made with a circle known as the Kew circle, two needles being always used, and the mean of the two results taken as the true value of the dip.

From this circle we have the following results:—

Year.	Mean dip.	Yearly secular change.
1856.	$68^{\circ} 27' \cdot 67$	'
1857.	$24^{\circ} 36'$	$3 \cdot 31$
1858.	$22^{\circ} 80'$	$1 \cdot 56$
1859.	$20^{\circ} 73'$	$2 \cdot 07$

If we take the mean of these three values of yearly secular change, and also include that between 1854 and 1855, we have a mean value of yearly secular change, for the period between 1854 and 1859, amounting to $2^{\circ}29'$, and this value will not be sensibly altered if we omit the observations between 1854 and 1855.

In 1859 it was resolved to substitute another circle for the Kew circle, as the action of the latter was not considered to be quite satisfactory; and accordingly since this date Barrow's circle No. 33 has

been employed, and monthly observations have been made with it, generally in the afternoon—two needles being used, as before.

From this circle we have the following results:—

Year.	Mean dip.	Yearly secular change.
1860.	68° 20' 21"	
1861.	18° 21'	2° 00'
1862.	15° 58'	2° 63'
1863.	12° 66'	2° 92'
1864.	9° 88'	2° 78'

exhibiting between 1860 and 1864 a mean secular change of 2° 58'.

It will be noticed from this, that the mean yearly secular change of dip at Kew appears to be greater from 1860 to 1864, a period of increasing disturbances, than from 1854 to 1859, a period of decreasing disturbances. Possibly the yearly decrement of dip has again begun to diminish, since the change from 1864 to 1865 is only 1° 32'. It is, however, premature to assert that this is the case, and it can only be decided by continuing the monthly observations. At all events the Kew observations agree with those at Toronto in indicating that the yearly change of dip contains the combined result of two things—namely, the true secular change and the change due to disturbance; and this ought to be borne in mind by future observers of this magnetic element.

GEOLOGICAL SOCIETY.

[Continued from p. 160.]

January 24, 1866.—W. J. Hamilton, Esq., President, in the Chair.

The following communication was read:—

“Notes on Belgian Geology.” By R. A. C. Godwin-Austen, Esq., F.R.S., For. Sec. G.S.

This communication related to the Upper and Lower Kainozoic formations of Belgium, in the following order:—1. The Polders, or sea-mud beds, and their equivalents. 2. The Campine sands, and Löss, or Limon de Herbaye. 3. The Boulder formation. 4. Cailoux Ardennais. 5. The Lower Kainozoic, or Crag.

The Polders, which form a belt along the sea-board of Belgium and Holland, occasionally running inland up the courses of rivers, as up the Scheldt to Antwerp, indicate an elevation of very small amount, corresponding to the raised estuarine and other beds around our own coasts. They are covered by dunes and drifted sands. A great deal of the fen-land at higher levels, with peat and bog-iron, belongs to the age of the Polders, and of still earlier times, inasmuch as the Polders very generally overlies a terrestrial surface. The Campine sands, which run parallel with the coast from North Holland to

wards Antwerp, but within the Polder-belt, were conjectured, from their composition and on other considerations, to have been derived from sands carried inland away from dunes of the Boulder-formation period. The Löss, which is of freshwater origin, resulted from the annual depositions of melted snow-waters. The dispersion of the Cailloux Ardennais was referable to another and earlier stage of a period of cold, and when the axis of the country had a greater relative elevation than at present. These views were supported by reference to the coast-section at Sangatte.

The Boulder formation proper is only slightly represented in some of the sections about Antwerp.

With respect to the Lower Kainozoic series, the author preferred the divisions proposed by M. Dumont (Scaldésien and Diestien) to the minute subdivisions of Sir C. Lyell and M. Nyst. The exceedingly narrow vertical dimensions of the Crag, and the manner in which, along the continuous sections now exposed, one bed of the Scaldésien Crag replaces another, are new facts, and preclude any systematic order of sequence, founded on percentage comparisons, from local assemblages of fossils.

The Antwerp Crag series presents two conditions of sea-bed:—a deepish-water and life-zone formation, corresponding to the ooze-depths of existing seas; this is the Diestien of Dumont, or Lower Crag: on an eroded surface of this, there occurs at Antwerp an upper series of coarser sands, shingle, and gravel, together with much which has been derived from the lower; this is the Scaldésien. The change from one to the other indicates a change as to depth over the Crag sea, and the result has been an admixture of the characteristic materials of distinct sea-zones.

The original boundary line of the Crag sea is traced, as also the great breadth of the drift-sand zone, over the Belgian area; this—coupled with the consideration that the Crag-sea waters on the continental coast-line nowhere came in contact with any beds older than Nummulitic, such as Tongrien and Bruxellien, even as high as Denmark, whilst on the English side, from Suffolk north, its coast-line was of chalk with flints—indicates a closed sea on the south, since only by such an arrangement could the flint-gravel be carried along.

The differences between the Crag-fauna of England and of Belgium were explained in accordance with bathymetrical distribution. The Scaldésien beds of Antwerp contain an assemblage which is composed in part of a littoral fauna, and in part of that of ooze-depths. The Red Crag of Suffolk differs from the Scaldésien in being more littoral in its forms, as also from containing the materials of a Bryozoan zone.

The Bolderberg beds, which afforded M. Dumont his evidence in favour of his "*Système Boldérien*," were shown to have been wrongly interpreted, and to belong to the Crag-sea accumulations.

XXXVII. *Intelligence and Miscellaneous Articles.*

ON THE CHANGES WHICH STRETCHING AND THE PASSAGE OF A
VOLTAIC CURRENT PRODUCE IN A MAGNETIC BAR. BY M.
VILLARI OF NAPLES.

MATTEUCCI states that if a bar of hard iron which is magnetized by a spiral is stretched, the magnetism of the bar increases. If the stretching ceases, the magnetism again diminishes. If the same experiment is made with soft iron, the reverse is the case. If the magnetizing spiral is not at work, stretching also causes an increase of magnetism, and relaxation (*nachlassen*) a diminution.

Wertheim has repeated Matteucci's experiments, but only confirms his results in case the magnetizing spiral is at work. He adds at the same time that the deflection of the galvanometer observed is smaller each time the stretching is repeated and the more the wire is straightened. He doubts, therefore, whether with a perfectly straight wire, stretching alters the magnetism of the wire.

As no further statements exist respecting stretching, it appeared desirable to make a few new experiments on the subject, in order to explain the difference between Matteucci's and Wertheim's experiments. The apparatus consisted, like Matteucci's, of two spirals, a magnetizing and an inducing one, in the latter of which a Wiedemann's mirror galvanometer, with sliding coils, was inserted.

In this method it is not the existing magnetism of the iron or steel bar which is measured, but the inducing-action which a change in it produces. But the subsequent investigation only refers to changes; and these may very well be measured by the induction which they produce.

The apparatus employed was the following:—

One spiral was firmly fixed in the other, and they were well fastened in a frame which stood on a table, and in such a manner that their common axis was from east to west. The galvanometer was at a distance of 4 to 5 metres from the spirals. The bar of steel or of iron which was to be used for experiment, was inside the inner spiral. At each end of the bar a thick brass wire was soldered, or else screwed. In order that neither these wires nor the bar to which they were fixed should move laterally, they passed through two pieces of wood which were firmly united with the frame in which were the spirals; and they also passed through corks which stuck in the ends of the inner spiral. One end was fastened to the stand by means of a screw, at the other was a rope which passed over a pulley, and by means of a lever could be stretched by 240 pounds.

The steel and iron bars, before being placed in the apparatus, were moreover straightened as much as possible, and after they were inserted were repeatedly drawn and, in order that they might be straight, were stretched by 40 pounds.

Both the spirals were coiled on brass sheaths slit lengthwise. The external magnetizing spiral, which consisted of covered copper wire 2·4 millims. in diameter, was 580 millims. in length by 225 millims. external and 110 millims. internal diameter. The interior spiral consisted of covered copper wire 1 millim. in thickness and 600 millims. in length; this had an external diameter of 30 millims., and an internal one of 19 millims.

The bars of iron and steel were all several inches shorter than the inner spiral.

The results of this investigation may be summed up as follows:—

With a *closed magnetizing spiral*, stretching and relaxing, the stretching produces an increase of the magnetic momentum up to a certain point; in like manner, opening and closing a voltaic current sent through iron or steel bars produces an increase of its magnetic momentum up to a certain amount.

If this limit has been attained, in further stretchings (as well as in openings and closing of the current which is passed through) the magnetism oscillates about this limit; in a steel or iron bar, stretchings diminish the magnetism, if it is thin and strongly magnetized, while relaxation of the stretching produces just as great an increase. But if the bar is thick and powerfully magnetized, an increase is produced on stretching, and on relaxation a diminution.

Yet, in transmitting a current, there is a difference between steel and iron when this limit has been attained. In the case of iron, there is an increase on closing the current, and on opening just as great a diminution, whatever be the direction of the current. Steel exhibits the same behaviour if the current enters at the south pole of the magnet; but if it enters at the north pole, there is conversely a diminution on closing, an increase on opening.

In the case of an *open magnetizing spiral*, stretching and relaxation produce a diminution of magnetism, to a certain amount. In like manner the passage of a current and its interruption produce a diminution of magnetism, up to a certain magnitude.

When this has been reached, on further stretching or on repeated opening or closing the current, the magnetism oscillates about this limit; and in soft iron stretching produces an increase, while relaxation produces just as great a decrease. Hard steel exhibits exactly the opposite behaviour; and between hard and soft steel all intermediate stages may be observed. Closing a current produces, in the case of iron, an increase of magnetism; opening it, just as great a decrease, whatever be the direction in which the current passes through the bar. The same is the case with steel if the positive current enters at the south pole.

Up to a certain limit stretching acts like a relaxation, and opening and closing a current like a mechanical agitation. So far, therefore, these first actions of traction and of the current are connected with other known phenomena. The phenomena, however, which occur after this limit is attained are new.

Those depend on another cause, which directs the molecules. This

needs, it is true, further investigation ; but that the molecules are in fact directed by a current which traverses the bar, follows especially from the circumstance, that on striking an iron bar through which a current has been previously transmitted, a current in the same direction is produced.—*Berliner Berichte*, July 1865.

ON THE HEAT OF FRICTION.

BY PROF. JOSIAH P. COOKE, JUN.

An accident to one of the large turbine-wheels employed by the Merrimack Manufacturing Corporation of Lowell has furnished a most remarkable illustration of the modern mechanical theory of heat ; and through the kindness of Mr. Isaac Hinckley, the accomplished agent of the Corporation, I have the pleasure of bringing the facts to the notice of the Academy. I cannot do better than to begin by reading Mr. Hinckley's own statement in a letter addressed to me, dated December 30th, 1864. The specimens referred to in the letter I have placed on the table for the inspection of the Academy.

"In accordance with your request, I herewith send you five pieces of metal once portions of one of our turbines. I have placed these pieces in the box in the same relative position which they occupied when they made a part of the turbine. To make my statement clear to you, I would refer to pl. 1 of Mr. Francis's admirable work, 'Lowell Hydraulic Experiments,' which you will find in the College Library. Our turbines are similar to the Tremont turbines therein shown.

"The turbine, of which these pieces were a part, is one of 250-horse-power under a fall of 32 feet, using 75 cubic feet of water per second. The wheel is of $58\frac{1}{2}$ inches diameter, with depth of float of 6 inches, and a velocity of 144 revolutions per minute. Its position is horizontal, and at a level of 3 feet below the surface of Merrimack River at its ordinary stage. It is mounted upon a vertical wrought-iron shaft 25 feet long and 6 inches diameter at smallest place. This shaft is fitted at its upper end with a series of disks by means of which it is supported in its box, which is again supported by a massive cast-iron frame. This frame supports the entire weight of the wheel and shaft. The shaft at its lower end is bored in the line of its axis to a depth of $5\frac{1}{2}$ inches to receive a steel pin of $17\frac{1}{2}$ inches in length and $2\frac{1}{2}$ inches in diameter, and which projects from the shaft 12 inches. The steady-pin has no function to perform other than that of restraining the shaft from lateral aberration. It is free to revolve in a box made of three pieces of case-hardened iron so placed in a cast-iron frame as to allow free play to the steady-pin and the free access of water to it. Each of these three pieces composing this box is kept up to its place by following-

screws working in the cast-iron frame which is bolted to the stone floor of the turbine-pit. In the Tremont turbine this floor is of wood, and in plate 1 the steady-pin is marked 'I.'

"The pieces sent are marked Nos. 1 to 5. No. 1 is the portion of the steady-pin which was nearest the shaft; No. 2 the other extremity of the same pin; Nos. 3, 4, 5 the three pieces of case-hardened iron forming the box, with portions of this steady-pin attached. You will at once perceive that this steel has been partially fused, and can thus account for its attachment to the iron.

"The facts are, that on noticing some irregularities of motion on the part of the wheel, it was stopped, and the water pumped from the pit until the floor was bare. Inspection showed that the following-screws had not done their duty uniformly; and the three pieces, Nos. 3, 4, 5, no longer preserved their proper relative positions, nor allowed free play to the steady-pin. The consequence was, an amount of friction causing heat sufficient to fuse steel, although the latter was immersed 3 feet deep in a raceway 10 feet wide, through which was passing 75 cubic feet of water per second.

"A similar accident happened thrice to our turbines, which are now, however, safely guarded against such mishaps."

There are two points in connexion with these facts to which I wish especially to call attention. In the first place, the weight of the wheel did not rest upon the surfaces of friction. The three pieces of case-hardened iron in their displaced position acted simply as a brake upon the revolving shaft; so that the heat must have resulted wholly from the destruction of mechanical motion: the immense moving-power of the wheel, instead of being directed wholly into its appropriate channel, was in part transformed into that mode of atomic motion called heat. In the second place, the temperature attained was at least the welding-point of iron, and this, too, although the heated metal was immersed in a stream of flowing water. It is undoubtedly true that the spheroidal condition of the water would greatly retard the loss of heat; but still the loss must have been exceedingly rapid. Now the loss, even at the highest temperature attained, must have been fully supplied by the heat generated during the same time; and this must, therefore, have been evolved with equal rapidity at the surfaces of friction. No change in the molecular condition of the iron, and no abrasion of the metal, is at all sufficient to account for this continuous, prolonged, and immensely rapid evolution of heat; and the facts force upon us the conclusion, that the destruction of mechanical motion is the one and only efficient cause. Moreover, if we admit the generally received principle of mechanics, that motion cannot be annihilated, the conclusion that heat is a mode of motion is equally irresistible. Lastly, it is evident that the facts here stated perfectly accord with the well-known experiments of Rumford and Davy; only, since the moving-power of the Merrimack turbine is so much greater than that employed by these distinguished experimentalists, the results which I have had

the pleasure of exhibiting are more striking and conclusive than any which have been previously obtained.—Silliman's *American Journal* for January 1866.

NOTE REGARDING THE DECREASE OF ACTINIC EFFECT NEAR THE CIRCUMFERENCE OF THE SUN, AS SHOWN BY THE KEW PICTURES. BY MESSRS. WARREN DE LA RUE, STEWART, AND LOEWY.

The remarks which we ventured to make in the last paragraph of our 'Results on Solar Physics,' recently published, have induced us to examine the Kew pictures, as regards the decrease of actinic effect from the centre to the circumference of the sun, to which decrease we may give the name of *atmospheric effect*, since it is without doubt caused by the presence of a comparatively cold solar atmosphere.

In conformity with our views, this atmospheric effect ought to be greater at the epoch of maximum than at that of minimum spot-frequency; and furthermore, if there is any reference to ecliptical longitudes in the behaviour of spots—that is to say, if at any time the spots on the sun attain their maximum at any ecliptical longitude—there ought (according to these views) to be a greater amount of absorbing atmosphere at the same longitude, since such an atmosphere is supposed conducive to the outbreak of spots.

There is reason to think that spots attain their maximum in the ecliptical longitude opposite to that where Venus exists; so that we might expect (according to these views) a diminution in atmospheric effect in the same longitude as Venus, and an increase in the effect in the longitude opposite to Venus.

If therefore Venus be at the longitude of the left limb of the sun, this limb should exhibit less atmospheric effect than the right limb; and if Venus be at the right limb, we should have most atmospheric effect at the left limb.

It is only the under-exposed pictures that are available for a research of this nature, since an over-exposure tends to do away with the atmospheric effect.

Without giving any hint of our views, Mr. Beckley was requested to select some of the last Kew pictures taken in 1859, a year of maximum spot-frequency, and to compare them with those taken in 1864 and 1865, periods of minimum spot-frequency; and he came to the conclusion that there was more atmospheric effect in 1859 than in the years 1864 and 1865.

Furthermore, Mr. Stewart, in conjunction with Miss Beckley, has looked over all the pictures taken at Kew from May 1863 to the present date; and this examination was made in such a way that the results could not derive any bias from the opinion of Mr. Stewart as one of the joint authors of the 'Researches on Solar

Physics,' above alluded to; for whenever the two observers disagreed, the picture was referred to a third person.

The results of this investigation are given in the following Table; and they are at least in conformity with our views, and not antagonistic, while at the same time the evidence is not sufficiently strong to establish conclusively the truth of an hypothesis:—

1863.	Left.	Right.	Equal.	
May	2	2	1	Venus from 80 to 90 degrees to the left.
June	2	1	2	
July	0	7	2	
August	3	3	1	" 50 to 60 " "
September	3	0	2	" 30 to 40 " "
October	1	0	2	" 10 to 20 " "
November	1	0	3	Venus in conjunction.
December	2	2	1	Venus about 5-10° to the right.
1864.				" " 20° to the right.
January	4	0	0	" from 30° to 40° to the right.
February	—	—	—	" about 45° to the right.
March	3	1	0	" " 90° "
April	3	3	4	" " 120° "
May	—	—	—	Approaching opposition.
June	1	1	7	Very near opposition.
July	4	3	7	Venus in opposition.
August	1	0	2	
September	3	4	2	About 150° to the left.
October	0	4	3	" 140° "
November	0	2	3	" 120° "
December	0	1	2	" 90° "
1865.				
January	0	2	0	Venus in conjunction.
February	0	1	0	
March	4	3	3	
April	0	2	11	
May	1	1	10	
June	2	0	1	Venus 90° to the right.
July	2	0	3	
August	6	1	5	
September	5	1	8	
October	8	2	5	

It thus appears, from the above Table, which is the result of a joint and careful investigation of the Kew pictures by Miss Beckley and Mr. Stewart, that—

(1) When Venus is considerably to the left, there is most atmospheric effect to the right.

(2) When she is in conjunction or opposition, there is a tendency to equality.

(3) When she is considerably to the right, there is most atmospheric effect to the left.—*Monthly Notices of the Royal Astronomical Society*, January 12, 1866.

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[FOURTH SERIES.]

APRIL 1866.

XXXVIII. *On the Composition of Forces.* By JOHN STEVELLY, LL.D., Professor of Natural Philosophy, Queen's College, Belfast*.

IT is well known that the very beautiful demonstration of "the parallelogram of forces" given by the immortal Laplace is too intricate, and depends too much on advanced knowledge of the differential and integral calculus and trigonometry, to be used in the instruction of mere beginners in the study of mechanical philosophy.

It has lately occurred to me that a most simple and almost immediate deduction from "Laplace's principle" would prove the parallelogram of forces in the case where the directions of the components contain a right angle—from which case to the general theorem, when the directions of the two components contain any angle at the material particle on which they simultaneously act, is but one easy step,—and that nothing but the extreme facility with which this master of analysis used the instruments of his art could have caused him to overlook this almost obvious and very simple method of proving what he takes about two quarto pages to deduce. It also proves at once the first and most important part of Poisson's proof of the parallelogram of forces, which, however, is scarcely anything more than a translation of D'Alembert's very elaborate geometrical proof into the language of algebraists and trigonometricians.

I shall therefore, with the permission of the editors of the Philosophical Magazine, give a simple geometrical proof of Laplace's principle, and the deduction from it which leads to the general theorem, premising one or two preliminary points for the sake of those readers who may be less familiarly acquainted with the subject.

* Communicated by the Author.

I. Two equal and directly opposite forces which act simultaneously on a material particle must manifestly each counteract any tendency to change its place given to the particle by the other; that is, they are *in æquilibrio*; and an absurd conclusion can be readily shown to follow from supposing any two forces acting together at a material point to be *in æquilibrio* but such as are both equal and directly opposed.

(a) Hence any number of forces which act simultaneously on a material point must have a single force, called their resultant, which, acting alone on that point, would produce in it the same tendency to move which they do acting together; for, however numerous and varied they may be, they can give to that point a tendency to move in only some one direction and with some definite energy. A single force, therefore, equal in energy to that and opposite in direction, might be assigned which would equilibrate against them all, and the single force equal and directly opposite to this would obviously have the same effect as all of them together.

Definition. Forces are represented by straight lines drawn either in the very direction in which they act, or along parallel lines in the direction along which they solicit the material point on which they act, lengths being taken along those representative lines which contain as many linear units and decimal parts of a unit as the force to be represented contains units of force and decimal parts of such a unit.

(b) Hence also it becomes obvious that if two forces which act together on a material point, say P and Q, have a resultant, say R, acting in a known direction in relation to the directions in which P and Q act, if instead of P and Q we apply to the same point two forces each equal to P and Q in magnitude respectively, but each directly opposite in direction to the one it replaces, the resultant of this latter pair is equal to R in magnitude, but exactly opposite to it in direction.

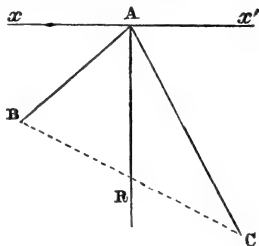
(c) Hence also it is easy to show that the representative of the resultant of a number of forces which all urge a material point to move in the same direction along the same right line is the sum of the lines which would represent each of them separately, measured in the same direction along the same line; and that the line which would represent the resultant of two forces which act on the same point in opposite directions is the difference of the two lines which would represent each separately, measured in the direction of the greater force; and hence also the representative of two groups in opposite directions is the algebraic sum in the direction of the greater group.

II. The ratio of two component forces which act together on a material point determines the direction of their resultant in

their plane, and within the angle contained by their directions; and the resultant of any other two components which act respectively in the same lines of direction and bear the same ratio to each other, will be in the same direction, and will bear to the resultant of the former pair the same ratio as that of either pair of homologous forces of the two pairs of components. (1) For let P and Q be two forces which act together on a material point in directions making any given angle, a second P and a second Q acting respectively in the same directions, would of course have an equal resultant, say R , acting in the same direction as before, and therefore $2P$ with $2Q$ would have a resultant $2R$ still in the same direction. The same argument applies to $3P$ with $3Q$, $4P$ with $4Q$, and nP with nQ , having for resultant nR in the same direction still. Again, $\frac{P}{2}$ with $\frac{Q}{2}$ must have $\frac{R}{2}$ for resultant in the same direction; for if not, the resultant R of P with Q must be different either in direction or magnitude, or both, from what it is. The same argument applies to $\frac{P}{3}$ with $\frac{Q}{3}$, and $\frac{P}{n}$ with $\frac{Q}{n}$, n' being any whole number, however large. From this it readily follows (as in Euclid V.) that P' with Q' in the same ratio as P to Q will have a resultant R' in the same direction and in the same ratio; thus $\frac{P}{P'} = \frac{Q}{Q'} = \frac{R}{R'}$. But (2) if we vary the ratio by adding to either P' or Q' the smallest additional force, the direction at least of the resultant will be changed, being brought from the previous direction nearer to that of the increased force, as it must be the resultant of the previous resultant and the added force.

Corollary. If the two forces be equal, their resultant must in this case manifestly have a direction which bisects the angle contained by their directions; and any other pair of equal forces acting in the same directions, but greater or less than those, will have a resultant whose direction will still be that of the line bisecting the angle, and greater or less than the former in the same ratio as the equal components are increased or diminished.

III. If two forces P and Q which act together on a material particle are represented in direction and magnitude by AB and AC respectively, which contain a right angle BAC , their resultant, whatever be its direction in the angle BAC , must be such that its square shall be equal to the sum of



the squares of P and Q, or $R^2 = P^2 + Q^2$; and therefore its magnitude must be represented by the length of BC, the line joining the extremities of the lines representing the two component forces.

For if, to fix our ideas, we suppose AR to be the direction of the resultant of P and Q represented by AB and AC, and at A erect xAx' both ways perpendicularly to AR, then, because BAC is by hypothesis a right angle, $xAB = RAC$, and $x'AC = BAR$. Hence the direction of P divides the right angle $xAAR$ into the same angles that AR divides the right angle CAB, and the direction of Q divides the right angle $x'ARR$ into angles equal to those into which AR divides the angle BAC. Hence, if we suppose P to be replaced by its two equivalent components in the directions AR and Ax, and Q by its two equivalent components in the directions AR and Ax', and call these respectively P', P'', Q', Q'', we shall have (by II.)

$$\frac{P''}{P} = \frac{Q}{R} \text{ and } \frac{Q''}{Q} = \frac{P}{R},$$

and therefore

$$P'' = \frac{PQ}{R} = Q''.$$

Hence the resultants of P and Q along Ax and Ax' are equal and opposite; and therefore, of the four which are together equivalent to P and Q and therefore to R, the components P' and Q', which act both in the direction AR, must be together equal to R. But again (by II.),

$$\frac{P'}{P} = \frac{P}{R}, \text{ therefore } P' = \frac{P^2}{R};$$

and

$$\frac{Q'}{Q} = \frac{Q}{R}, \text{ therefore } Q' = \frac{Q^2}{R};$$

hence

$$P' + Q' = R = \frac{P^2}{R} + \frac{Q^2}{R},$$

and

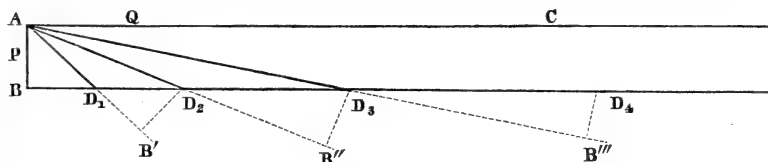
$$R^2 = P^2 + Q^2.$$

Hence, whatever be the direction of R (and that has yet to be determined), its magnitude is represented by the length of BC, which joins the extremities of AB and AC, which represent P and Q both in magnitude and direction; and this is also equal to the diagonal of the rectangle whose sides are AB and AC*.

* This, with a portion of the preceding, constitutes what is familiarly known as "Laplace's Principle."

IV. We shall now proceed to prove that the direction of the resultant of two forces P and Q , whose directions contain a right angle and which act together on the material point A , is that of the diagonal of the rectangle whose sides represent P and Q respectively; or, what comes to the same thing, if the two forces P and Q which act together at A are represented in direction and magnitude by the two sides of a right-angled triangle taken in order (AB and BD), the hypotenuse AD (taken not in order, or) acting from A to D represents the direction of their resultant, as its length has been already proved to represent its magnitude.

(1) Let $P=Q$, and let AB represent P , and $BD_1=AB$



represent (by parallelism) the force equal to P which acts in the direction of AC . Then, since the line AD_1 (the diagonal of the square, or the hypotenuse of the triangle ABD_1) bisects the angle BAC , it is the direction of the resultant (by Cor. of II.), and its length (by III.) represents the magnitude of the resultant on the same scale as $AB=BD_1$ represent $P=Q$.

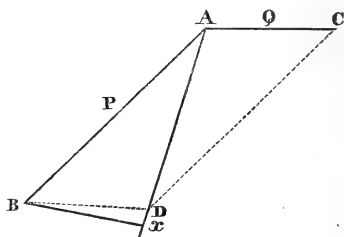
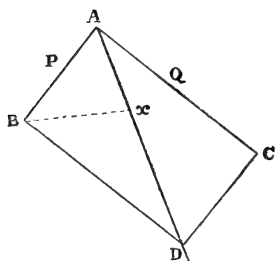
(2) Let now D_1D_2 be made equal to AD_1 , and join AD_2 . Now let a force Q , acting along AC with P still along AB , be such as in magnitude to be represented by BD_2 , then shall AD_2 represent, in both direction and magnitude, the resultant of P and Q represented respectively by AB and BD_2 . For produce AD_1 , and from D_2 let fall on it the perpendicular D_2B' . Then, since the triangle $D_1B'D_2$ is equiangular with ABD_1 , two forces represented by D_1B' and $B'D_2$ would have a resultant represented by D_1D_2 . If, now, along with the two forces represented by AB and BD_1 , we introduce a force represented by D_1D_2 , and along with AD_1 the two forces D_1B' and $B'D_2$, we shall have the two forces AB with BD_2 , equivalent to the two AB' with $B'D_2$; and therefore some one line, both in direction and length, must represent the two resultants of the two pairs of forces acting each together at the material point A . But since the two triangles ABD_2 and $AB'D_2$ are equiangular, and the angle at A in the one equal to the angle at D_2 in the other, if we were to suppose any other line than AD_2 to be the direction of the resultant of AB with BD_2 , we should be compelled (by II.) to believe a line lying at an equal angle on the other side of AD_2 to be the direction of the resultant of

proved that AD_n is the resultant of AB with BD_n , by introducing with AB and BD_n the subtractive force represented by $D_n x$, and with AD_n the subtractive force represented by $D_n B''$ with $B'' x$, we again see that AB with Bx must have the same resultant as AB'' with $B'' x$. Hence AB' with $B' x$ must have the same resultant as AB'' with $B'' x$, and this can have no other representative than Ax without contradicting either what has been already proved (in II.), or what has been now proved. If, now, z coincide with x , it is proved that the resultant of AB with Bz is represented by Az . If not, z must lie either between D_{n-1} and x or between x and D_n . Now draw Ax' bisecting the half angle within which z lies, and, proceeding exactly as before, we show that Ax' is the resultant of AB with Bx' . If then, again, z coincide with x' , we have proved what we require. If not, we again draw Ax'' bisecting the half angle within which z is found; and so on without limit by continual bisection until either z lie in the last drawn bisector, or it appears that Az is the limit towards which the last bisector may approach closer than by any angle however small. In the limit, then, Az represents both in direction and magnitude the resultant of the two forces represented by AB with Bz acting together on the material point A^* .

* Since the above was printed, I have found that (2), (3), and (4) may be most materially shortened and simplified, thus:—(2) and (3). Since in (1) we have seen that AD_1 represents both in direction and magnitude the equivalent or resultant of the two equal forces represented by AB and BD_1 , if with each of these we introduce a force acting at A represented by $D_1 D_2$, we shall see that the resultant of P acting with Q , now represented by BD_2 , must be the same as that of the two equal forces represented by AD_1 and $D_1 D_2$ acting together at A . But since AD_2 bisects the angle $D_1 A C$, its direction must be that of the resultant of the two latter (by Cor. II.), and therefore of the resultant of P and Q represented by AB and BD_2 ; and (by Laplace's Principle III.) the magnitude of that resultant is represented by the length of AD_2 ; AD_2 then in this case represents both in direction and magnitude the resultant of AB and BD_2 . Similarly, by introducing along with each of these at A another force coinciding in direction with Q , represented by $D_2 D_3$, the new resultant of P with Q now represented by AB and AD_3 is shown to be represented in direction and magnitude by AD_3 . And so on for $AD_4 \dots AD_n$, n being any whole number however large.

Again, (4) suppose we have now proved that the resultant of P and Q represented respectively by AB and BD_{n-1} is represented by AD_{n-1} both in direction and magnitude, and that the resultant of AB and BD_{n-1} is AD_{n-1} , and that of AB with BD_n is AD_n . Let Ax , as above, bisect $D_{n-1} A D_n$, and introduce with the first pair and their resultant a force at A represented by $D_{n-1} x$, then the resultant of AB with Ax will be the same force as the resultant of AD_{n-1} with $D_{n-1} x$. Also, by introducing with the latter pair (AB with BD_n) and their resultant (AD_n) the subtractive force at A represented by $D_n x$, we shall again have the resultant of AB with Bx the same as that of AD_n with $D_n x$; therefore this resultant must be the

V. The general theorem is now easily proved in the ordinary way. Let AB and AC or BD represent any two forces P and



Q acting together on the material point A ; then shall AD represent both in direction and magnitude their resultant. For from B let fall the perpendicular Bx on AD , then may P be replaced by the two forces represented by Ax with xB , and Q (represented by BD) by the two forces represented by Bx with xD , and P with Q together by the four represented by Ax , xB , Bx , and xD ; of these four, xB equal and opposite to Bx produce no effect and may be removed, and therefore Ax with xD or AD must represent the entire resultant of P with Q .

This demonstration seems to me the simplest extant, with the exception of the dynamic proof given in Dr. Robinson's too much overlooked treatise on Mechanics, the fallacy in which I have never been able to detect. The foregoing static proof, however, is shorter and more direct than Poisson's or D'Alembert's, and does not need the introduction of the differential or integral calculus as used by Laplace, nor the principle of transference of forces as used in the very beautiful proof of Duchayla.

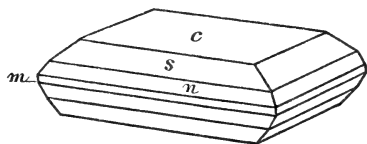
same as both AD_{n-1} with $D_{n-1}x$, and of AD_n with D_nx . But since Ax bisects the angle $D_{n-1}AD_n$, $\frac{AD_{n-1}}{AD_n} = \frac{D_{n-1}x}{D_nx}$ (Euclid VI. 3); hence $\frac{AD_{n-1}}{D_{n-1}x} = \frac{AD_n}{D_nx}$; therefore (by II.) the resultant of AD_{n-1} with $D_{n-1}x$ must make the same angle with AD_{n-1} as that of AD_n with D_nx does with AD_n . Hence Ax must be the direction of both these (as $D_{n-1}Ax = D_nAx$); and therefore Ax must be the direction of the resultant of AB with Bx ; and Laplace's principle proves the length of Ax to represent its magnitude. Exactly similarly we prove the same for Ax' and Ax'' , and so on to the limit Az , as in the text above.

XXXIX. *On the occurrence of Wulfenite in Kirkcudbrightshire.*

By Dr. HEDDLE*.

GREG and Lettsom doubt whether or not Wulfenite may rank as a British species. In Thomson's 'Mineralogy,' vol. i. p. 565, we find the following remarks on a specimen (a *single specimen*) in the possession of the Stockholm Academy:—"It was ticketed *lead-spar*, from Mendip, near Churchhill in Somersetshire; it was chiefly carbonate of lead, but it contained two portions of a yellower colour than the rest, which attracted the particular attention of Berzelius†. One of these being examined by the blowpipe, proved to be molybdate of lead. The other portion was an oxido-chloride of lead."

This, so far as I know, is the only published record of the occurrence of this substance in Britain. Lately a new pit was sunk for some 30 fathoms at the "South of Scotland Mines" at Lackentyre, near Gateshead in Kirkcudbrightshire; and among several interesting minerals brought me thence by Mr. James Russel of Airdrie, there was a single specimen of the molybdate. The associated minerals are galena, carbonate and phosphate of lead, and cupreous calamine; the molybdate occurs in well-pronounced and unusually brilliantly polished crystals of about one-eighth of an inch in size; the forms are *csn* and *csnm*; the faces *n* and *m* are very narrow: the crystals are thin, transparent, and bright yellow.



Lackentyre.

The pit in which this specimen was found proved unproductive, and has been abandoned; there is thus little hope of others being obtained.

XL. *On the Heat disengaged by Induction-currents, and on the relation between this disengagement of Heat and the mechanical force employed to produce it.* By M. E. EDLUND‡.

WHEN a closed conducting wire is brought near a galvanic current, an induced current is produced in the former, the direction of which is such that it would give rise to a repul-

* Communicated by the Author.

† Kong. Vet. Acad. Handl. 1823, p. 184.

‡ The original memoir was published in the *Comptes Rendus de l'Académie des Sciences de Stockholm*, 1864, p. 79. The above article is the translation of an abstract contained in the *Bibliothèque Universelle*, December 20, 1865.

sion between the two currents. If, on the contrary, the conductor is removed, the induced current is in the opposite direction, from which would result an attraction between the two currents. Induction, therefore, in the two cases is accompanied by an expenditure of mechanical force. If, on the contrary, the galvanic induction is produced by a change in the intensity of the inducing current, without any change in the relative positions of the two currents, there is no loss of mechanical force. In this respect, then, the two kinds of current are dissimilar. At first sight it would appear that an approximation of the two currents ought to have the same effect as an increase in the intensity of the inducing current, and that their separation would have the same result as a diminution in the intensity of the inducing current—in fact, that approaching the induced circuit to, or removing it from, the inducing current, would really only produce an increase or a decrease in the intensity of the latter. But if the principles of the mechanical theory of heat can be applied here, when the induction is accompanied by an expenditure of mechanical force, the development of heat must necessarily be greater than when the induction arises from a change in the intensity of the current; and this excess must be proportional to the work performed.

Induction-currents offer, therefore, a suitable means for confirming experimentally the general validity of the mechanical theory of heat.

These considerations led M. Edlund to study more closely than had hitherto been done the thermal effects of induction-currents. We shall give a brief analysis of his paper, in which he commences by stating the laws of induced currents, and passes then to the relation which exists between the heat produced and the mechanical force exerted.

II.

An induction-current can scarcely have any other properties than those of an ordinary galvanic current whose intensity is constantly changing. It was easy to foresee, therefore, that the heat which it produces at a given time must be proportional to the square of its intensity at the same time. The means hitherto employed in regard to this question, when constant galvanic currents were concerned, are insufficient for demonstrating the perfect accuracy of this proposition in the present case. On the other hand, Weber's electro-dynamometer furnishes a very simple method for this object. This instrument, as we know, consists of two coils of silk-covered copper wire, one of which is fixed, and the other suspended to two fine silver wires so as to be capable of oscillating freely about the position of equilibrium determined by

the two wires. When the internal coil is in equilibrium in this position, its axis is at right angles with that of the external coil. If now the same current is sent through both coils, the moveable one must experience a deviation in virtue of their mutual action. This is determined in the usual manner by means of a mirror, a telescope, and a graduated scale. If i denote the intensity of the current at a given time, and dt the element of time, the deviation must be proportional to $\int i^2 dt$, the limits of the integral being the time comprised between the commencement and the end of the current. The heat developed by this induction-current must be proportional to this quantity, in case the supposition we have made is correct.

A coil consisting of several turns of silk-covered copper wire was connected with a battery; the current could be opened or closed by means of a toothed wheel worked by a handle held in the hand. The toothed wheel had fifty teeth, so that at each complete turn the current was broken or closed one hundred times by means of a spring on the side of the wheel. In this induction-coil another similar one was introduced, its external diameter being just sufficient to permit this. The two ends of this latter induction-wire could be united at will either to the dynamometer or to two small perpendicular brass rods, between which was stretched a thin platinum wire which the induced current could traverse. To estimate the heating effect produced by the induced current in the platinum wire, a small thermoelectric pile was adapted to the latter, the current of which passed by means of two copper wires to a mirror-galvanometer at a suitable distance, and which had served in previous researches. The deflections of the galvanometer could be read in the ordinary manner, by means of a graduated scale and a telescope. The thermoelectric pile was the same as that previously employed by the author in his investigations on the development of heat produced by the change of volume of solid bodies*; in some experiments an analogous pile was used, in which certain modifications were introduced. The platinum wire and the pile were placed under a bell-jar to preserve them from currents of air.

In order that the induced current may produce a heating of the platinum wire capable of developing an appreciable thermoelectric current, the toothed wheel must be turned a great number of times. When the development of heat due to the induced current was to be measured, the wheel was turned with a uniform velocity until the needle was at rest and exhibited a constant deflection. This was observed. It is clear that, when this state has set in, the wire loses in a given time, by radiation and con-

* Phil. Mag. S. 4. vol. xxiv. p. 329.

duction, as much heat as the induced current produces in the same time. But the heat which escapes is proportional to the excess of temperature if the latter is inconsiderable. Hence the heat developed in a given time by the induced current is proportional to the excess of temperature. M. Edlund has shown by previous experiments that the thermoelectric current, and therefore the deviation which it produces in the galvanometer, are proportional to the excess of temperature*, provided the latter is small. It follows from this that the deflection of the galvanometer is proportional to the heat disengaged in a given time by the platinum wire. After having shown the truth of this proposition by special experiments, and proved moreover by other experiments that the indications of the dynamometer are proportional to $\int i^2 dt$, the author made several experiments to determine the heat disengaged by induced currents. The following are some of them:—

In a first series of experiments the dynamometer was in connexion with the internal coil; the dynamometer was then removed and replaced in the circuit by a platinum wire. After having determined the development of heat in the platinum wire by means of the thermoelectric pile and the galvanometer, several observations were again made with the dynamometer. These experiments were made with inducing currents of different intensities. The following are the means of the results obtained:—

Deflection of the dynamometer.	Deflection of the galvanometer for heat.
<i>y.</i>	<i>x.</i>
98·8	113·3
52·4	60·1
73·75	82·8
40·9	46·8
18·3	20·3

If we calculate the deflections (*x*) due to the thermoelectric current, supposing them proportional to those of the dynamometer (*y*), according to the equation $x = 1·14 y$, we get

Calculated.	Observed.	Difference.
$x = 112·6$	113·3	—0·7
84·1	82·8	+1·3
59·7	60·1	—0·4
46·6	46·8	—0·2
20·9	20·3	+0·6

* Vide *suprà*.

There is thus perfect proportionality between the quantities of heat disengaged by the induced currents, and the deflection which these produce in the dynamometer. The same result was obtained when the duration of the induced currents was altered, the intensity of the inducing current being kept constant. With this view a second special spring was fixed to the toothed wheel, so as to close another circuit at the moment at which the battery was cut off by the other spring at the opening of the current. The extra current, produced in the induction-coil on opening the battery, had time to act, and had the effect of making the diminution of the inducing current on opening take place far more slowly than in case there was no secondary circuit. This arrangement produced no change in the induction-current produced by closing the circuit. In order, lastly, to vary the duration of the induced current, an extra induction-coil was introduced at first into the inducing current, in which was a soft iron cylinder. The effect of this latter on the inducing current was to diminish its increase on closing. This induction-coil was so distant from that in which was produced the induced current to be measured, that no action could take place between them. In order that the quantity of heat produced should not be too small, the secondary circuit was removed for this case. In the three modes of working, the intensity of the inducing current was virtually constant.

Without accessory circuit and without electro-magnet,

Dynamometer.	Heat.
71.75	110.0

With accessory circuit and without electro-magnet,

30.83	48.0
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Without accessory circuit but with electro-magnet,

60.01	88.25
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If we calculate the means by the equation $x = 1.512 y$, we get

Calculated.	Observed.	Difference.
$x = 46.6$	48.0	-1.4
90.7	88.25	+2.45
108.5	110.0	-1.5

We see that in this case also the quantities of heat developed by the induced current are proportional to the deflection of the dynamometer. This result proves that the heat disengaged in each moment by the induced current is proportional to the square of the intensity of this current in the same moment, or, what is the same thing, that the heat developed by the entire

induced current is proportional to $\int i^2 dt$, taking the commencement and end of the induced current as limits of the integral.

III.

An induced current naturally exerts an action on the principal inducing current. Hence are formed in the latter induced currents of a higher order, which, according to their direction, add themselves to or subtract from the principal current. Thus the disengagement of heat in the inducing current while induction is taking place may be different from that which takes place during the same length of time but when no induction takes place. Hence, if we are to measure the heat disengaged by induction, we must not only measure the quantity of heat which the induction-current develops in a given time, but also that which the inducing current develops in the same time, first without induction, and then while it is inducing. The experiments were arranged in the following manner with a view to this object.

The toothed wheel was turned with a constant velocity, making an entire turn in a second; and on the stroke of the fortieth second the deflection in the galvanometer produced by the thermoelectric current was observed. To determine the disengagement of heat due to the induced current, the platinum wire to which the thermoelectric pile was attached was interposed in the induced current; it was then removed and placed in the principal inducing current. After that, the disengagement of heat was examined anew in the platinum wire, first when the induced circuit was open and when no induction took place, and then when it was closed, that is, while under the inducing action of the principal circuit. Thus a measure of the heat developed by the induction-current in the platinum wire during twenty seconds, was obtained; and the same method gave a measure of the development of heat due to the principal current in the same platinum wire and in the same time, at first without induction, and then during the induction exerted by the principal current.

We shall not mention here the experimental difficulties which the author had to overcome, nor the proofs he adduces in support of the exactitude of the method he followed, and shall only cite the final results.

In the first series of observations the production of heat due to the induced current in the platinum wire, and received by the thermoelectric pile, gave a deflection in the galvanometer of 28.75. The heat developed by the principal current in the same wire, but without induction, gave 188.84, and when the

principal current induced, 177·26. The difference of these two last numbers, or 11·58, represents the lessened disengagement of heat by the principal current when the latter induces a current in another conducting wire. But the numbers 11·58 and 28·75 (obtained by the induced current) only represent the disengagement of heat in the platinum wire, and give no account of the disengagement in the other parts of the two circuits. Hence by comparing these two numbers we cannot know whether an induction of this kind really produces heat. But for the same current the disengagement of heat is proportional to the resistance of the circuit, and that not merely for metallic conductors, but also for liquids. Hence, if we multiply 28·75 by the resistance of the induced circuit, and 11·58 by the resistance of the principal current, the first of these products will represent the total heat disengaged in the induced current, and the second the difference in the heat developed in the principal circuit when the latter induces and when it does not. If M is the resistance of the principal current, we find by experiment that the resistance of the induced current is 0·4405 M . These two quantities of heat may therefore be represented by $28·75 \times 0·4405 M$, and $11·58 M$. The first gives 12·66 M , and may be regarded as being quite equal to the second; from which it follows that this kind of induction does not give rise to a production of heat.

Two other series of observations led to the same result; in the first, the heat disengaged by the induced current was 12·36 M , and the difference of the heat disengaged by the principal current during induction and without induction was 13·21 M . In the second series 9·52 M was obtained for the first arrangement, and 9·63 M for the second. These results prove *that when induced currents are produced in a closed circuit by opening and closing the principal inducing circuit, the induction gives rise neither to an increase nor to a diminution of heat. The quantity of heat developed by the induced current is equal to the lessened production of heat which takes place in the principal current owing to this induction.*

IV.

In order, by bringing a secondary circuit near an inducing current or by removing it away, to obtain an induced current so strong that the heat which it disengaged could be measured, a special apparatus was constructed. It consisted of a fixed coil of copper wire covered by silk, the interior diameter of which was 200 millims. In the inside of this was another, also covered with silk, which by suitable mechanism could be made to rotate rapidly about an axis in the plane of the first coil. The ends of

the wire of the moveable coil were in metallic connexion with the ends of the axis of rotation. These were provided with springs that could be connected with wires through which passed the induced current produced by the rotation. The principal inducing current, whose intensity was constant, always passed in these experiments through the fixed external coil. When the internal coil was rotating, it produced an induced current in the fixed coil. It is readily understood that the induced current is always in a direction such that the mutual action of this current and of the principal inducing current exerts a resistance to the rotatory motion, and that this resistance must be overcome by the mechanical force which makes the coil rotate. An expenditure of mechanical force therefore accompanies the induction; and this expenditure of mechanical force, as can easily be seen, is proportional to the square of the intensity of the principal current so long as the velocity of rotation is constant.

The experiments were made in the following way. The ends of the inner coil were in the first place connected with the platinum wire on which the thermoelectric pile was placed. After that the interior coil was made to rotate, and when the velocity of rotation was constant (forty-five turns in a second), the exterior coil was connected with the voltaic battery, the rotation being continued for thirty seconds; the battery was disconnected, and after a lapse of twenty seconds the deflection produced in the galvanometer by the thermoelectric current was read off. This deflection was a measure of the quantity of heat disengaged by the induced current during thirty seconds. That being done, two similar experiments were made, with the simple difference that in the principal current was inserted the platinum wire with its thermoelectric pile. In one of two series the internal bobbin was rotating; in the other it was at rest; so that in one case there was induction, and in the other not. By means of the two latter series a measure was obtained of the quantity of heat disengaged in thirty seconds by the principal current in the case in which there was induction, and in that in which there was not. Now it was found that the principal current produced the same quantity of heat whether it did or did not act inductively. In one series of observations a deflection of 166.6 was obtained without, and of 166.9 with induction. The disengagement of heat in the induced circuit in the same series was 49.3. Hence induction of the kind in question produces an excess of heat, and this excess is exactly equal to the heat which the induced current disengages. But the heat disengaged by the induced current is proportional to the square of the intensity of the induced current; and this, in

accordance with the general laws of induction, is proportional to the intensity of the principal current. The excess of heat obtained by this kind of induction is proportional to the square of the intensity of the principal current. On the other hand, the square of the intensity of the principal current is proportional to the mechanical force used in surmounting the resistance which the inducing and induced currents exert on being approached to or removed away from each other. Hence it follows that, *when induced currents are produced by approaching or separating the induced and the inducing circuit, heat is produced by induction. In this case the production of heat is proportional to the mechanical force exerted in approaching the two circuits or in separating them.*

The author has proved by a mathematical deduction, which cannot be reproduced here, that the variation in the disengagement of heat of the principal current arises from induction-currents of a higher order which are produced by the primary current.

XLI. *On the Diminution of Direct Solar Heat in the Upper Regions of the Atmosphere.* By J. M. WILSON, M.A., F.G.S., Fellow of St. John's College, Cambridge, and Mathematical and Natural Science Master of Rugby School.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the March Number of the Philosophical Magazine Professor Tyndall has drawn attention to a note of mine in the previous Number, on an observation of Mr. Glaisher's respecting the readings of the shaded and exposed thermometers at great heights.

Professor Tyndall restates my conclusions, and, not admitting them, however fairly drawn, to be representative of natural facts, suggests a possible source of error in the observations arising from a supposed small capacity of the black-bulb thermometer for absorbing the invisible solar rays. I will not occupy your space by quoting Professor Tyndall's statement, but referring to it would remark that, even granting any amount of transparency to the invisible rays on the part of the black bulb, the phenomenon noticed by Mr. Glaisher cannot be thus accounted for, as far as I see at present, without adopting my conclusions which Professor Tyndall quotes. He is doubtless right in saying that the indications of the black-bulb thermometer are more

Phil. Mag. S. 4. Vol. 31. No. 209. April 1866.

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or less delusive, or that we must be careful not to misinterpret its readings; but the special failure of the black bulb which he suggests as possible would not explain this perplexing phenomenon.

For if the black bulb were opaque to the visible and transparent to the invisible rays, the case would be as follows:—There issue from the sun calorific rays, as is commonly supposed, of various qualities, which we may class as visible and invisible rays. Both fall on our atmosphere and pass through it, suffering loss as they descend, but the invisible rays suffering most, from their greater absorption (as is known) by the aqueous vapour in the atmosphere. Now if the black bulb were opaque to, and therefore absorbed, the visible rays, but were transparent to, and therefore, we will suppose, totally reflected the invisible rays, the two thermometers at great heights would differ only by the amount due to the direct action of the visible rays. Now they differ, as Mr. Glaisher assures us, by *nil*; therefore the effect of the visible rays on the black bulb at great heights, where the visible rays are at a maximum, is *nil*; and therefore, of course, if they suffer no change in their transit through the atmosphere, their effect at all points in their path down to the surface of the earth is *nil*. The invisible rays, moreover, as they descend are more and more absorbed, and therefore affect both bulbs equally, and have less and less effect by direct action on the exposed bulb, if by this hypothesis they have any effect at all. Why, then, the two thermometers should differ so much on the earth's surface it is not easy to see. It cannot be in consequence of one of them being exposed to the visible rays, for they produced no effect above; nor in consequence of the invisible rays, on this hypothesis, for, if they affected it at all, they would affect it most where they themselves are greatest, at great heights.

I am obviously not supposing that Professor Tyndall made this hypothesis with a view to explain the phenomenon in question; but it is as well to point out that his remarks, though very valuable as indicating to meteorologists a possible limitation of the powers of the black bulb which may have escaped them hitherto, do not at all set this question at rest. But his hypothesis is somewhat startling, for one imagines glass transparent to brilliant, and opaque to obscure heat; and it would be strange if blackening the glass reversed both these properties at once.

I am aware that the pyrheliometers of Herschel and Pouillet have given grounds from which it has been concluded that if the atmosphere were removed we should receive two-thirds as

much heat again from the sun as we actually do receive. And I know too, by personal experience, as does Professor Tyndall, how at great heights one feels sensations commonly attributed to solar radiation; and the conclusions in my last paper, which seemed to contradict these and multitudes of other facts which occur to everyone, surprised me and staggered me, I confess. But the truth is that the contradiction lies between, not the facts, but the received interpretations of the facts; and when this is the case, the interpretations must be reexamined; and the process is one which leads to discoveries.

Whatever hypothesis is offered, the fact remains, that at great elevations the shaded and exposed bulb show very nearly the same very low reading, so low that it may be presumed that they would show the same reading exactly if they could be taken to a sufficiently great height. Mr. Glaisher, I will repeat, reasserted this, in the most explicit manner, as based on very numerous and entirely concordant observations both of the black-bulb thermometer and water-actinometer.

The only explanation that I can suggest that is based on known physical actions is the following.

If the black-bulb thermometer does not indicate the direct radiated heat, it may be that it loses it by instantaneous radiation. It is contrary to our experience that this instantaneous radiation is due to diminished pressure; but it may be due to the nearly total absence of aqueous vapour. It is the presence of the atmosphere, and especially its aqueous vapour, that protects the earth from radiating away its heat rapidly by night. It may be the presence of aqueous vapour round the bulb of the exposed thermometer that prevents it also from radiating away its heat by day, and that prevents our being chilled by radiation, even in sunshine, down to the temperature indicated in the shade, a temperature which, but for the aqueous vapour, would be much lower than it is. Where, then, the exposed bulb is protected from loss by the opaque aqueous vapour, it will show the heat it receives; where it is unprotected, it will lose this direct heat as fast as it receives it, and indicate the temperature of the air. Should this be the case, it will be an interesting result of Professor Tyndall's discoveries of the high radiating and absorbing powers of aqueous vapour, that it protects the earth and us from instantaneous radiation (that peculiar sensation we feel at great heights and call burning)—that, in a certain sense, if it does not create heat for us, but for it we should have no heat to lose.

It is quite unnecessary to point out the difficulties in accepting this; but to this we are driven if the observations are correct,

the instruments not entirely fallacious, and heat exists in stellar space.

Professor Tyndall says, in his 'Heat considered as a Mode of Motion,' p. 389, "The withdrawal of the sun from any region over which the atmosphere is dry must be followed by quick refrigeration. The moon would be rendered entirely uninhabitable by beings like ourselves through the operation of this single cause: with an outward radiation, uninterrupted by aqueous vapour, the difference between her monthly maxima and minima must be enormous." If the radiation were instantaneous, Mr. Glaisher's observation indicates that the surface would not be heated; it may not be safe to extend this to rough surfaces, as to sand, as it is conceivable that the reciprocal radiation among the particles may be communicated to the mass; but except on some such insecure ground as this (rejecting, as before, the hypothesis of the non-existence of heat, as such, in stellar space), I see no way for reconciling the above inference of Professor Tyndall, or the similar remarks by Herschel (*Outlines of Astronomy*, §. 431), with Mr. Glaisher's observation.

I have selected the comparison between the bright and dark halves of the half moon as an example which gives a pointed illustration of the difference between my view and the received opinions: it seems paradoxical; but is it contrary, not only to opinions and to inferences, but to facts? In suggesting it I confess I see less ground for hesitation in the physical difficulty of the view than in the mere fact of differing from two such authorities as I have quoted against myself, from whom I can only differ with a mental reservation that further thought and more study on my part would probably bring me to adopt their conclusions. I trust that Professor Tyndall will continue to give his attention to this interesting subject.

I remain, Gentlemen,

Your obedient Servant,

JAMES M. WILSON.

Rugby, March 10, 1866.

XLII. *On Molecular Physics.* By Prof. W. A. NORTON*.

[Continued from vol. xxx. p. 289.]

TERRESTRIAL MAGNETISM.—In accordance with the ideas already advanced as to the essential nature of electrical excitation (vol. xxx. p. 107), we may conceive that the earth may derive its magnetic condition from currents developed in its crust by the impulsive action of the æther of space upon the molecular atmospheres†. Both the rotatory and orbital motions of the earth may be concerned in the production of such currents. The rotation of the earth should develop currents at each point of its surface, starting in a direction parallel to the equator, and flowing from east to west. Also, if we consider the points of the earth lying on or near the meridian whose plane passes through the sun, and designate the velocity of the earth in its orbit by V , and that of rotation by v , the absolute velocity of the points in question will be $V+v$ on the side opposite to the sun, and $V-v$ on the side toward the sun. The current in the former case, due to the velocity $V+v$, will run from east to west; and that in the latter case, due to the velocity $V-v$, will run from west to east. The intensity of the former may be represented by $m(V+v)^2$, and of the latter by $m(V-v)^2$. Taking the difference between these two expressions, we obtain as the excess of the intensity of the east and west current over the other, $4mVv$. Such, then, would be the intensity of the effective current at any point, due to the combination of the velocity of rotation and the velocity in the orbit. At points of the earth's surface at any moment in the vicinity of the meridian at right angles to that just considered, the currents developed, so far as they originate in the tangential action of the æther, will be wholly due to the earth's rotation. At certain distances from this meridian, the component of the orbital velocity, in a direction parallel to the surface, will exceed the velocity of

* From Silliman's Journal for January 1866.

† As intimated in a former part of this memoir, the priority in the publication of the general theory that the earth derives its magnetic condition from its collision with the æther of space is conceded to Professor Hinrichs, of the Iowa State University, and formerly of Copenhagen. But the idea was no less an original one with the author; and his conception of the essential nature of dynamic electricity, and the magnetic condition of the earth, and his physical theory of terrestrial magnetic phenomena as resulting from the same supposed original cause, are materially different from the views advanced by Professor Hinrichs. It will be seen also that the theory now presented is but the complement to a previous series of researches upon terrestrial magnetism, prosecuted at intervals through a period of about twenty years, and a natural offshoot from the theory of molecular physics propounded in this paper.

rotation; and the current developed on the side nearest the sun will run from west to east. East and west currents will therefore be developed at every place during the greater part of any single day, and the opposite current will originate only during a certain interval of time before and after the middle of the day. Also the east and west current will be more intense than the opposite current developed in corresponding positions. At the close of a day, a certain resultant current for each place should remain, running from east to west. As the obliquity of the ecliptic to the meridian at the hour of noon at any place is continually changing during the year, this resultant current must be continually changing its direction. This change of direction may be represented by supposing the current developed each day to lie in a small circle traced around the point 90° from the ecliptic, on the meridian 90° from the station, and that this magnetic pole is carried through the geographical pole in the course of a year. Under this idea, each place will have its separate oscillating magnetic pole. At the end of a year these diverse directions of current will also have a resultant; and by considering contiguous places, it may be seen that these annual resultants will lie for a certain district in parallel small circles having a common pole. If we confine our attention to points on the equator, and suppose the magnetic properties of the crust of the earth to be the same at all points, it is plain that every such pole will coincide with the geographical pole, since the annual resultants would be coincident with the equator. But should the conductibility of the earth be unequal in different directions, the final currents developed in such directions should be unequal, and hence the annual resultants should be variously inclined to the equator, and their poles have diverse positions. At points situated without the equator the unequal intensities of the currents developed at different seasons of the year will determine at each locality an annual resultant having a certain direction, generally more or less inclined to the equator.

In what precedes, we have confined our attention to the action of the æther in directions tangential to the earth. Such currents should be chiefly of the nature of galvanic currents—that is, proceeding from molecule to molecule. Those which result daily from the combined effect of the two motions of the earth will originate in lines parallel to the ecliptic, and follow the directions (or at least in their mean course) of circles traced around the position of the pole above mentioned, on the earth's surface, on the day considered. These may be called ecliptic currents. The currents due to the earth's rotation alone will be of a similar character, and follow circles parallel to the equator. These two sets of currents, especially the former, play the prominent part

in originating and maintaining the normal magnetism of the earth, and determining the secular changes that occur in its distribution. The currents resulting from the earth's rotation can serve only to maintain a uniform normal condition of such currents previously developed. But the æther of space also impinges normally upon the forward side of the earth. The principal effect of this mode of action, that we have occasion to consider, will be the origination of a series of waves of translation in the sea of electric æther that pervades the interstices of the molecules, spreading out from the most advanced point of the earth. They may be conceived to consist of an endless number of linear currents radiating in great circles from that point. This description of currents exhibit their effects conspicuously in the daily and annual variations of the declination and directive force of the needle. They conspire with the others, and to a certain extent modify them, and originate similar ones.

It should be added that the more permanent magnetic forces developed by the currents above considered may consist, in a great degree, in secondary currents excited within the molecules of the earth. The author's former investigations accord with this view. In a memoir "On Terrestrial Magnetism," published in *Silliman's Journal*, vol. iv. p. 1, a theory of the magnetic action of the earth was propounded and discussed, based upon the fundamental assumption that "every particle of matter at the earth's surface, and to a certain depth below the surface, is the centre of a magnetic force exerted tangentially to the circumference of every vertical circle that may be conceived to be traced around it." This tangential action upon the north pole of the needle was conceived to be directed downward on the north side of the particle, and upward on the south side (see p. 4 of the paper just referred to). Now, if we regard the particles of the earth's crust as so many separate magnets—magnetized by electric currents developed as we have been considering—we are conducted by an inevitable sequence to this fundamental basis of the theory in question; for all such molecular magnets will at each station have their axes perpendicular to the resultant currents traversing that station to which the magnetization is due. The north end of every such indefinitely small magnet will exert an attractive force upon the north end of the needle, and the south end will exert an equal repulsive force upon the north end of the needle. Since the lines of direction of these forces will not be strictly coincident, their resultant will bisect the outer angle between them, and so be perpendicular to the line proceeding from the centre of the molecular magnet. A series of such minute magnets extending for a small distance will form a magnet of finite length, the entire action of which

will be sensibly the sum of the individual actions, and will be perpendicular to the line proceeding from the middle of the magnet. The directive action of the earth will be virtually this.

This being allowed, it follows, as deduced in the former paper, that, except in high latitudes, the needle will be perpendicular to the lines of equal molecular magnetic intensity; also that the horizontal directive force exerted by the earth will be proportional, or nearly so, at each station to the molecular magnetic intensity; and the vertical force approximately proportional to the difference of these intensities on one side and the other of the lines of equal force. It may be added here that the above conception brings our theory into essential correspondence (from the mechanical point of view) with Gauss's, and thus that the conclusions of his memoir become deducible from the present physical theory.

If we conceive the magnetic force of the earth to be wholly due to the *direct* action of the electric currents circulating from molecule to molecule, the force exerted by each element of the current should be of the same character, and have a similar direction to that in the case just supposed. But since the resultant currents are shifting their position from year to year, it follows that they may differ somewhat from the lines of equal molecular force. In the sequel we shall in general, for greater simplicity and distinctness of conception, regard the magnetic action of the earth as due to the primary currents, developed as before explained. If these give place, either wholly or in part, to molecular currents, the results will be essentially the same.

Distribution of Terrestrial Magnetism.—The inequality in the distribution of the magnetism of the earth, upon the same parallel of latitude, may be supposed to arise from differences of conductivity in different parts of the earth. It is conceivable that such differences may exist as a consequence of the existence of two great systems of continental elevations, and that the magnetic condition of the earth may be represented by supposing that two sets of currents, originating in these elevations, are superimposed upon those which are due to the undisturbed condition of the crust of the earth. But there is another conception that may be formed of the possible origin of the unequal distribution of the earth's magnetism, which does not involve the supposition of unequal conductivity. It is that the present magnetic state of the earth originated at a remote period in the history of the earth, when it was still in the process of condensation, and its period of rotation was much longer than at present. It will readily be seen that at every epoch during this transition period, in which the period of rotation was the $\frac{1}{n}$ part

of the tropical year, n being an even number, the same region of the earth's surface would at the close of each successive year be, for a considerable interval of time about the autumnal equinox, on the opposite side of the earth from the sun, and thus would come to be traversed by strong currents running from N. of E. to S. of W. (p. 273); also that at each successive vernal equinox the same region would be on the side of the earth turned towards the sun, and therefore in the most favourable position for the currents already developed at the autumnal equinox to be reinforced by the new currents*. The systems of currents thus originating at such successive epochs would not in general be coincident; but it will be seen in the sequel (p. 278) that each system should become subject to a motion of revolution under the operation of the new effective currents annually developed, and that the annual rate of displacement should be different for each system, unless their currents should be of equal intensity, which would be in the highest degree improbable. Now, if the shifting movements of the different sets of currents were unequal, the tendency should have been, in the lapse of ages, to bring them all into coincidence, or to consolidate them into one system in each hemisphere. In the light of Gauss's investigations into the magnetic state of the earth, we may conclude that the earth has actually reached this period of its magnetic history.

At the epochs for which n was an uneven number, two systems of currents should have been developed, one at each equinox; and the intensity of each of these would have been much less than that of the single system (the sum of the two equinoctial systems) answering to the epoch when n was an even number. These separate systems of currents should therefore, by reason of their secular movements, have tended to become incorporated with the other more effective ones, which would have been displaced more slowly.

It will be seen, in another connexion, that the magnetic state of the earth experiences certain changes from year to year in response to the varying magnetic and electric condition of the sun's surface. We may then conclude, from our present point of view, that the existing system of magnetic currents should bear the traces not only of the changes through which the magnetic condition of the earth has passed, but also of the mighty changes that have passed over the face of the sun.

From our present point of view we may discern the probable link of connexion between the magnetism and the temperature of the earth. In the paper already referred to (p. 267), a mathe-

* It is here implied that the more effective currents are developed at the equinoxes; in explanation of this see pp. 273, 276.

matical exposition was given of the formal relations subsisting between the principles of magnetism and heat in the crust of the earth, based upon certain mechanical ideas. We have already seen (p. 267) that the fundamental ideas then assumed are in essential accordance with the present theory of the origin of terrestrial magnetism. It may now be added that the mathematical relations shown to subsist between the intensity of the magnetic action and the temperature may be seen to have a physical basis. The dependence here alluded to arises from the fact that the electric currents developed by the impulsive action of the æther of space within the crust of the earth must, to a certain extent, pass off in the form of heat, and that the earth may derive a large portion of its heat from this source. Inequalities in the mean temperature of the earth's crust at equal distances from the equator should result from inequalities of elevation, &c., and from variations in the intensity of the resultant currents traversing the localities. It will be readily seen that if the inequalities of the mean temperature of the crust of the earth resulted entirely from the heat developed by the supposed action of the æther of space, the distribution of the temperature and magnetism would entirely correspond, that the poles of greatest cold would coincide with the magnetic poles, and the thermal equator with the magnetic equator.

Periodical Variations of the Magnetic Elements.—In a paper published in Silliman's Journal, vol. xix. p. 183, the author undertook to show that these variations are such as should result from two supposed sets of currents traversing the photosphere of the earth, or two corresponding sets of currents traversing the earth's crust. These currents were called respectively *radial* and *ecliptic*—the radial currents radiating from the region of the photosphere most directly exposed to the impulsive action of the sun's rays, and the ecliptic originating on the side of the earth toward the sun, and in directions parallel to the plane of the ecliptic. We have now to observe—(1) That the ecliptic currents running from east to west, formerly supposed to be developed in the earth's photosphere, have their counterparts in currents running from west to east in the crust of the earth, and developed by the orbital motion of the earth on the side nearest the sun. (2) That the orbital motion of the earth develops, within the mass of the earth, currents running from east to west on the side of the earth opposite to the sun. (3) The impulsive action of the æther upon the forward parts of the earth, as it advances in its orbit, must originate currents radiating from those regions over the earth, and will especially give rise in the early morning hours to currents running toward the north in latitudes lying to the north of the ecliptic, which will deflect the needle

toward the east. Strictly the two sets of ecliptic currents, the one having an easterly and the other a westerly trend, will be developed at various points on one side and the other of the circle of intersection with the earth of a plane passing through the most advanced point and the geographical pole. The special currents developed on the circle perpendicular to this will have the greatest intensity on the side opposite to the sun, as already shown. The effects of the diverse currents, originating in the impulsive action of the æther of space upon the preceding half of the earth, are conspicuously observable in the variations of the declination and horizontal force during the last half of the night and the earlier part of the day. As the day advances, the radial photospheric currents (see Silliman's Journal, vol. xix. p. 190) come into more effective action, and greatly modify the magnetic variations that would result from the currents just mentioned. They augment the diminution of the horizontal force in the forenoon, and deflect the needle further to the west at midday*. They are also the principal cause of the increase of the horizontal force in the afternoon. The change of the hours of the morning maxima and minima with the seasons is mainly a consequence of the changes experienced, during the year, in the position of the circle of the earth perpendicular to the radius of the earth's orbit at 6 A.M., with respect to a meridian passing through the most advanced point of the earth's surface at that hour. The circle in question coincides with the meridian at the two equinoxes, is inclined $23\frac{1}{2}^{\circ}$ to it on the west side at the summer solstice, and under the same angle on the east side at the winter solstice. In consequence of the change of position of this circle, the radial currents tend to alter the critical hours above referred to. For the rest, it should be observed that the phenomena all show that the temporary currents by which they are produced do not come into most effective action until a certain interval of time after the moment of most intense excitation, because, doubtless, of the residual currents that continue in action with diminishing energy.

The two sets of currents that have been specified afford a complete explanation of the observed periodical variations of the declination and directive force of the magnetic needle. In considering their separate action it is to be distinctly observed, (1) that the currents produced in the earth's crust by the impulsive action of the æther of space are developed at each station between the hours of midnight and noon, though the currents thus excited will be propagated on and produce a certain effect at other stations before midnight and after noon; (2) that the

* The special effects here alluded to, and in general the effects referred to in what follows, are those observed in our latitudes.

radial photospheric currents are chiefly effective between the hours of 6 A.M. and 6 P.M., though their influence extends, especially during the summer, into the earlier and later hours of the night. In their effect upon the declination, the marked tendency of the first set of currents is to deflect the needle toward the east for a certain interval of time before and after 6 A.M., while the conspicuous tendency of the second is to deflect the needle toward the west for a certain interval about the middle of the day. Another effect of the latter set of currents is, when the sun is north of the equator, to augment the morning easterly deflection produced by the former currents. In their effect upon the horizontal force of the needle, the tendency of the radial photospheric currents is to diminish its intensity between midnight and noon, and increase it between noon and midnight; but these effects are especially produced during the forenoon and afternoon. On the other hand, the other set of currents tend especially to augment the horizontal force during the latter half of the night, and to diminish it during the forenoon. The morning increase of the horizontal force is more conspicuous during the winter than during the summer months, for the reason that the diminishing action of the radial currents in the morning hours is greater in summer than in winter.

In studying the *Annual Variations*, we must take note of any changes that may occur during the year in the intensity of the two sets of currents by which all the phenomena are conceived to be produced. In fact both sets of currents have varying effective intensities. In these latitudes the radial currents are most effective toward the summer, and least effective toward the winter solstice—as a natural result of the varying positions of the point of the earth's photosphere directly underneath the sun*. The other set of currents have a maximum of effective action at the autumnal equinox, and, considered individually, a minimum at the vernal equinox†. For at the autumnal equinox the most advanced point of the earth's surface upon which the

* The precise epoch when the radial currents are most effective should vary with the latitude of the station. It is plain that near the tropic of Cancer it should be some weeks before or after the summer solstice; for at the summer solstice, at the hour of noon, the currents or waves that reach the station from the different points of the photosphere that receive the sun's rays should exactly neutralize each other. The epoch or epochs in question it is obvious should approach the summer solstice as we recede from the torrid zone. The observations made at Philadelphia on the horizontal force indicate that the radial currents are most effective in determining the diurnal variation of the horizontal force about a month and a half before and after the summer solstice.

† Just as with the radial currents, the epoch of maximum effect must vary with the latitude, and in the lower latitudes should occur before and after the autumnal equinox.

impulses of the æther fall normally will lie $23\frac{1}{2}^{\circ}$ to the north of the equator, and at the vernal equinox it will lie $23\frac{1}{2}^{\circ}$ to the south of it. Owing to the annual change in the intensity of the radial currents, the diurnal variations, both of the horizontal force and declination of the needle, that occur during the forenoon and afternoon are greater in the summer than in the winter. The maximum variations occur after the summer solstice, and the minimum after the winter solstice. By reason of the annual change in the effective action of the other set of currents, the morning variations of the horizontal force and declination (*i. e.* for a certain interval before and after 6 A.M.) are greater at the autumnal than at the vernal equinox. The more effective action of these currents at the autumnal than at the vernal equinox is conspicuously seen in the higher maximum of the horizontal force at 5 A.M. to 6 A.M., and the lower minimum about 10 A.M. (See Prof. Bach's 'Discussion of the Magnetic Observations made at Philadelphia in 1840 to 1845,' p. 45.)

We should here call attention to a special fact from which it results that the currents developed by the æther, both on the side of the earth towards the sun and on the opposite side, are especially effective about the equinoxes. It is that for a considerable period before and after these epochs, such currents, excited at any one place, have very nearly the same direction, and so cooperate more effectually. (See additional remark on p. 276.)

Among the annual variations of declination may be specified an easterly movement of the needle at the hour of 6 A.M. from the winter to the summer solstice. The author has already shown, in his previous paper (*Silliman's Journal*, vol. ix. p. 196), that such an effect should result from the action of the radial currents. Another annual variation that has been detected is an augmentation of the mean monthly intensity of the horizontal force, from winter to summer. (Professor Bache's *Discussion*, &c., p. 59.) To understand how this may result, it is to be observed that since the effective radial currents steadily increase in intensity from winter to summer, and since the action in the afternoon of each day is to augment the horizontal force, and in the forenoon to diminish it, whatever effective residual current may remain as the result of the entire action of the currents in question during a single day, must have the direction of the currents that augment the force. By the continual accumulation of such residual currents, there must accordingly be a tendency to an increase in the intensity of the horizontal force from winter to summer.

But the other system of radial currents should also cooperate with these in producing variations in the intensity of the horizontal force from one month to another. Since the effective

action of these increases, as we have seen, from the vernal to the autumnal equinox, and an action to augment the horizontal force each day is followed by one to diminish it, the tendency of the daily accumulation of residual currents should be to diminish its mean daily value from the vernal to the autumnal equinox. Such a tendency does in fact manifest itself. As the result of the observations at Philadelphia already referred to, the mean monthly value of the horizontal force was 0.0018 of its absolute value less in September than in March*.

In the former memoir it was maintained that the *Irregular Disturbances* of the magnetic needle might be satisfactorily explained if we admit the existence of occasional photospheric currents proceeding from various points over the preceding and following hemispheres of the earth, and that the ordinary region of maximum excitation lies in the plane of the ecliptic from 60° to 90° to the west of the point of the earth's surface that has the sun in the zenith, and that the region diametrically opposite to this is a secondary region of special excitation. In special instances the point of maximum excitation may have other positions, nearer the meridian in which the sun lies. An adequate cause for such occasional currents may be found if we conceive that they result from the penetration from time to time into the earth's photosphere of bodies of auroral or vaporous matter, expelled from the sun, and arriving with absolute velocities ordinarily less than that of the earth in its orbit. The photospheric currents may be conceived to result either directly, from the impact of the auroral matter, or indirectly, from electric discharges at special localities within the photosphere, consequent upon the reception and distribution of such bodies of matter.

* There are still other operative causes that tend to produce annual variations of horizontal force, viz. all the changes that occur annually in the effective action of the ecliptic currents, whether developed in the crust of the earth or its photosphere. The general causes of change are (1) a variation in the velocity of the earth in its orbit; (2) a variation in the direction of the currents excited; (3) a change in the extent of the portion of each parallel of latitude that is exposed to the impinging action of the æther or auroral matter; (4) *a change in the direction of the progressive motion of the solar system as compared with the direction of the orbital motion of the earth*. The epochs of maximum and minimum dependent upon the first cause should fall near the solstices; and those dependent upon the third and fourth causes should fall near the equinoxes. The effects of the second cause will vary with the locality. *The currents due to the general motion of the solar system are most intense just before the vernal equinox, and least intense just before the autumnal equinox.*

The conjoint action of the two systems of currents that have been under consideration, in determining the annual variations, might be strikingly exemplified by considering those which occur at the intertropical station of St. Helena.

This conception of the origin of the irregular disturbances links them theoretically, as they are in fact, on one side with the physical changes observed in the photosphere of the sun, and on the other with the auroral phenomena that occur in the photosphere of the earth. It contemplates the coruscations of the aurora and the sympathetic tremblings of the magnetic needle, as but one phase of the "magnetic storm" of subtile vapour that descends upon the earth from the regions of space.

One of the most conspicuous facts relative to the disturbances under consideration is, that the disturbances of the horizontal force that diminish its intensity prevail, at all hours, over those which augment it. This fact may be attributed, from our present stand-point, to the circumstance that the descending masses of auroral matter, in receiving the velocity of rotation of the photosphere of the earth, must generate electric currents or progressive waves directed toward the west. There is still another effect that theoretically should result from the arrival of these cosmical masses. The electrical excitation that should thereby be produced in the photosphere will act indirectly, in a greater or less degree, upon the earth's surface, and develop currents running over it in every direction from the locality immediately underneath the region of excitation in the upper atmosphere. The increase in the morning maximum of horizontal force in the years of greatest disturbance gives indication of the existence of this effect. The tendency of such currents will be almost identically the same with the currents we have supposed to be directly developed in the earth's crust by the impact of the æther of space.

The electrical action upon the crust of the earth here considered may be in a great degree direct rather than inductive; that is, the penetration of the subtile cosmical matter into the earth's photosphere may occasion streams of electricity in the direction of the impact that may penetrate the atmosphere and take effect upon the earth's surface. The physical cause here supposed to be in operation should cooperate with the others that have been noticed in determining regular variations of the declination and directive force that would be observable in the mean daily variations for a month or a year, even after these have been freed from the greater disturbances.

It is conceivable that the effects which have been ascribed to the radial photospheric currents might be produced by an analogous system of currents within the earth's crust directed toward the region directly underneath the sun. But no plausible cause can be assigned for the existence of such currents, since, if the sun be supposed to produce tides in the vast sea of

electric æther that pervades the earth's crust, and thus originate the currents supposed, the consequent effects upon the declination and horizontal force should be of the same character, if not of equal amount, at midnight and at noon. Besides, the moon by this sort of action should produce greater effects than the sun. The moon, as a matter of fact, does exercise a disturbing action upon the magnetic needle, but the perturbations produced by it have only been detected by the closest scrutiny.

We may here take occasion to remark that the lunar-diurnal variations of the declination and of the horizontal force are, in their nature, such as should result from a tidal action of the moon upon the terrestrial sea of electric æther. Thus there should be theoretically a maximum of west declination at the upper culmination, or thereabout, and another maximum at the lower culmination. There should also be a maximum of horizontal force a few hours after each culmination, and a minimum a few hours before each culmination; for the rise and fall of the electrical tide should be attended with currents, or rather waves of translation, setting from all directions toward the point underneath the moon, or a point somewhat in advance of this, and also toward the diametrically opposite point.

Secular Variations.—The secular changes experienced by the declination and directive force of the needle appear to be *the natural consequence of the continual operation of the physical process by which the earth was originally magnetized*. It will be recollected that this consists principally in the development of ecliptic currents on the side of the earth furthest from the sun, which have a greater intensity than the oppositely directed currents developed on the side toward the sun; also that these preponderating currents which originated at any station at the solstices run from E. to W., while those developed at the vernal equinox proceed from S. of E. to N. of W., and those developed at the autumnal equinox from N. of E. to S. of W. It is also to be observed that, *in the northern hemisphere, the currents which originate at the autumnal equinox exceed in intensity or quantity those which originate at the vernal equinox*, for the reason that a greater portion of each northern parallel of latitude is exposed to the impulsive action of the æther. Now conceive all the currents in question that originate during the year at any station to be decomposed into two, one running from E. to W., and the other from S. to N. or from N. to S. It will be seen that the annual resultant of the one set of components will constitute a current from E. to W., which will be equal to the sum of the individual components; while that of the other set will be equal to the excess of the currents that run from N. to S. over those

that run from S. to N. These general facts being borne in mind, it may be seen that the secular variations of the declination result from the combined operation of two causes, viz. :

(1) The prevailing annual action of the resultant E. and W. current, or of the resultant N. and S. current, according to the declination of the needle ; except when the declination is easterly, when the two currents will cooperate.

(2) The varying action of the impulses proceeding from the resultants of the new currents and those previously existing, shifting and changing in intensity from year to year, which run through all the places that lie on the east and west sides of the magnetic meridian of the station.

Let us conceive the diverse directions of the needle on different meridians to be represented by a sinuous curve, alternately concave and convex toward the north, to which the needle is perpendicular—a certain point of the concave portion being on the meridian of Philadelphia, and a point of the convex portion on the meridian of London. Now, confining our attention to the first operative cause on the concave part where the declination (E. or W.) is small, it is plain that the N. and S. current should prevail, and therefore the needle have an annual westerly movement. But at a point of the ascending curve where the declination (W.) is large, the other current should prevail, and the needle turn toward the east. Both of these cases are represented by the present secular variations at Philadelphia and London. On the higher part of the curve, where the declination (E. or W.) is small, the N. and S. current should prevail again, and the needle be deflected toward the west. The neutral or transition-points in the curve should fall at about equal distances on opposite sides of the point of maximum declination (W.).

If we follow the curve ascending toward the west from the line of no declination on this continent, both sets of currents will cooperate, and the needle should turn toward the west, as it now does throughout the United States. It appears, then, that throughout Western Europe and the United States the actual progressive movements of the needle are precisely those which should result from the operation of the first cause above mentioned ; that is, from the direct action of the new currents developed at the station of the needle.

To understand how an alternation of movement may occur at a given station, we must consider the probable and possible effects of the other general cause. Under the operation of the first cause the present westerly movement at Philadelphia should continue until Philadelphia has magnetically the position of the more westerly of the two neutral points above mentioned.

Phil. Mag. S. 4. Vol. 31. No. 209. April 1866.

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But the needle will not, in fact, remain stationary when this position has been reached; this could not be the case unless the effects of the varying resultants of the new and old currents should exactly counterbalance each other. In reality those on the east side should preponderate over those on the west side, because they will be more displaced and the currents of impulses proceeding from the same number of points will correspond more nearly in direction on the east than on the west side. The tendency of the second general cause should then be to give the needle at Philadelphia a motion toward the east in the magnetic position in which it would otherwise remain stationary.

If we now revert to London as a type-station for Western Europe, the present easterly movement of the needle should continue until the magnetic position of the more easterly of the two neutral points so-called is reached. But at this position the resultant currents at places lying to the west should, in the existing condition of the currents of the eastern continent, preponderate over those lying to the east, and the easterly movement should therefore continue. The continued operation of the second general cause may thus keep up an easterly movement until the needle attains to a certain easterly declination. But the direct tendency to a westerly movement that increases as the easterly declination becomes greater, must ultimately prevail, and the needle begin to turn toward the west.

It is obvious that the general result is the same as if the whole system of currents were gradually transferred to the west; or the representative sinuous curve had such a motion, its folds at the same time changing more or less. Or rather, to obtain a comprehensive view of the entire process, we should conceive of a system of such representative curves traversing the earth's surface at various distances from the equator, and suppose the whole system to be carried bodily toward the west.

To explain completely the secular variations, especially of the *horizontal force*, we must take into account another cause in operation not yet mentioned. It is that the resultant currents at any station may either be increasing or decreasing in intensity from year to year, for the reason that the annual diminution of intensity of currents already existing may be over-compensated by the new currents, or the reverse*. During the period of over-compensation or of increasing intensity, the period of the secular change of declination should increase, and decrease in the

* A tendency to a diminution of the horizontal force may arise from two causes, viz. a gradual decline of existing currents, and an increase in the ecliptic photospheric currents developed by the impact of the auroral matter received from the sun.

succeeding period. Since it appears, from Mr. Scott's discussion of the secular variations (see Report of Coast Survey for 1855, p. 337), that the secular period is shortening on the western coast of the Atlantic, we have to infer that we are at present in that magnetic phase in which the reinforcement of intensity from the new currents is less than the annual diminution. In this circumstance we have the probable explanation of the annual diminution of the horizontal force in the United States and Canada. An increase of the photospheric currents may cooperate.

Another general principle should be had distinctly in mind in this connexion: it is that the action of the auroral matter received from the sun upon the photosphere of the earth develops there a system of currents, the tendency of which should be the reverse of that of the corresponding system continually developed in the crust of the earth by the æther of space. The relative direction in which the solar matter approaches the earth is also approximately the same as that of the impulsive action of the æther—only that, in proportion as the velocity of recess from the sun is greater, the direction of approach is displaced toward the sun. As already intimated, the impinging solar matter also develops radial currents by direct action propagated to the crust of the earth. This effect we have recognized in the partial dependence of the morning variation of the declination, and of the horizontal force, upon the eleven-year period of the sun's spots. It is also strikingly manifest in determining the principal deflections of the needle during an aurora borealis, at the same time that the ecliptic and equatorial currents from east to west developed in the photosphere have the effect of diminishing the horizontal force. This supposed action of the solar matter upon the crust of the earth may arise either from the direct propagation of the impulses, as already intimated, or, more probably, from an increase in the density of the æther, resulting from the acceleration of the fall of the matter in question, produced by the earth's attraction.

The secular variations should also be dependent in some degree upon the electric currents due to the solar matter. In fact there is abundant evidence of such dependence. The annual rates of variation of all the magnetic elements vary during the eleven-year period, as they should do upon this supposition. Thus the tendency to a westerly deflection of the needle, and to a diminution of the horizontal and vertical forces, is least in the year of minimum spots and magnetic disturbances. It is interesting to observe, in the Philadelphia Observations, how manifestly this minimum tendency existed in the case of each of the three elements in the years 1842 and 1843. Another evidence of the dependence in question is afforded by the fact that the

annual rate of the secular variation of declination in this country reached its maximum about the year 1855, and that this is near the maximum epoch of the secular period of the sun's spots. In Europe the tendency of the same general cause is to make the secular rate the least at the same epoch. In this way probably it has happened that the increasing secular rate of the easterly movement there has become nearly constant*.

Observation has furnished the means of testing the explana-

* From our present stand-point we may obtain a distinct view of the origin of the diverse luminous phenomena of the aurora borealis, as well as of the attendant magnetic phenomena. We may perceive that the aurora is a combined magneto-electric and electromagnetic phenomenon; that the auroral light results from electric discharges along the lines of magnetic polarization that traverse the masses of solar matter while passing over from the preceding to the following side of the earth's photosphere; that the discharges are in a great degree due to the demagnetizing action of the electric currents developed by the solar matter impinging upon the preceding side of the photosphere, but in part also to a direct disturbance of the electric equilibrium along the lines of polarization by these currents or by the free electricity in the photosphere. We here allude especially to the more conspicuous auroras. It is conceivable that, should there be an intermission in the reception of auroral matter from the sun, or the supply feeble, the currents continually excited in the earth's crust by the æther of space may, by augmenting the intensity of the earth's magnetism, originate currents in the photosphere directed upward instead of downward. Such effects should be especially observable in the regions of greatest directive force. It is to be observed that the tendency of the demagnetizing action accompanying the more conspicuous auroras, with the attendant electric currents, is to disperse the auroral matter, and in this way to occasion its expulsion to an indefinite distance, under the operation of the repulsive force of the earth exerted upon single molecules or minutely divided masses (see Silliman's *Journal*, vol. xxxviii. p. 70). The decrease of the earth's magnetizing action cooperates in this.

There are several important probable inferences that may be drawn from the preceding discussion, which it may be advisable to state here very briefly.

1. The sun must have become magnetized after the same manner as the earth, by reason of its rotation, and of its motion of rotation combined with its progressive motion through space. As in the case of the earth, there must be a continual development of new currents tending to exalt its magnetic state. These new currents, by this mode of action, should condense the auroral matter of the photosphere along the lines of polarization, and so develop both light and heat. The spots on the sun are probably due to an inverse effect (that is, demagnetizing and dispersing) produced by the electric currents directly developed in the photosphere, by the descent into it of cosmical matter as the sun moves forward in space. According to this, the faculæ and accompanying dark spots have a similar origin to terrestrial auroras. Upon this theory, the dark spots should be wanting at the magnetic equator and at the poles. They should also be mostly confined to low latitudes (heliographical).

It is probable that a large fraction of the heat by which the temperature of the body of the sun is maintained is the result of the continual recurrence of the process of magnetization by the impinging action of the æther

tion we have given of the progressive change of declination. Dr. Lloyd, in his discussion of the Dublin Observations (between 1840 and 1843), has established that the needle at Dublin has, from the vernal equinox until after the summer solstice, a motion in a direction opposite to the annual progression, and a motion in the other direction from the autumnal to the vernal equinox. The discussions of the observations at Philadelphia and Toronto have revealed a similar law at those stations, though the direction of the annual progression is reversed. Now at Dublin the new currents developed at all seasons tend to give the needle an easterly deflection, except near the autumnal equinox, when their effect upon the declination will be slight. For the currents will run from S. of the magnetic E. to N. of

of space (p. 270). The penetration of cosmical matter into the photosphere is another source of heat.

2. Similar inferences may be drawn with respect to the magnetic and thermal condition of the planets; and an approximate estimate may be made of the comparative condition of the different bodies of the solar system.

3. The continual development of heat in the entire mass of the earth, by the action of the æther, is probably the origin of those subterranean titanic forces which have so repeatedly, in past geological ages, fractured and upheaved certain portions of the earth's crust, and whose effects are now observable in earthquakes and volcanic eruptions. Upon this idea there should probably be certain lines of upheaval corresponding to the magnetic currents in some of their shifting and comparatively stationary positions.

4. The rotating and revolving nucleus of a comet should become magnetized and heated in the same manner as the earth and the sun, both in its mass and photosphere. In this fact we have the apparent origin of the formation and detachment of successive nebulous envelopes, and of the emission of luminous jets from the nucleus, the process of detachment and indefinite expulsion being the same as already alluded to as in operation in the photosphere of the earth (p. 280). The same process attends the formation of the solar spots, and originates streams of nebulous matter seen in the zodiacal light. The residual cometary phenomena which remain unaccounted for by Olbers and Bessel's theory, as applied and amplified by the author (see *Silliman's Journal*, vols. xxvii., xxix., and xxxii. [2]), may be understood, in their minute details, in the light of the present conception.

5. It may be added in confirmation of the theory of the continual descent of auroral matter, derived from the sun, into the earth's photosphere, that the diurnal variations of the electric tension near the earth's surface are in accordance with the idea that free atmospheric electricity, for which no adequate terrestrial cause has yet been ascertained, is derived from the auroral matter thus received. Also the diurnal variations of the barometer are other observed effects that should result, on mechanical principles, from the same general cause.

Again, the diminution in the hourly fall of the temperature during the latter part of the night, for which no sufficient meteorological cause can be found, would seem to afford direct evidence of the heating effect that has been attributed to the resisting impulses received from the æther of space.

the magnetic W., except at the autumnal equinox, when they will be nearly perpendicular to the needle. In the annual inequality, therefore, the needle should be in its most easterly position at the vernal equinox, when the currents will be most oblique to the needle, and at its most westerly position toward the autumnal equinox. At Philadelphia and Toronto the secular change is due to the excess of the N. to S. currents, from the summer to the winter solstice, over the S. to N. currents from the winter to the summer solstice. Under the influence of these currents the needle should be in its most westerly position near the winter solstice, or near the close of the period during which the N. to S. currents are developed, and at its most easterly position near the summer solstice. The observations at these stations give results in entire accordance with these theoretical conclusions. But for the influence of the currents at other localities, the amount of the inequality should be equal to the annual secular change. This was the case at Toronto (each 2') in the years from 1845 to 1851. At Philadelphia the annual progression in 1843 was 4'4, more than double the annual inequality (2'). This must be attributed to the preponderating action of the currents traversing those localities at which the needle was turning toward the west.

Unequal Magnetic Intensities of the two Hemispheres.—This has its origin in the *unequal absolute velocities of the earth, near the equinoxes, resulting from the progressive motion of the solar system*. A calculation from the most reliable data gives for the ratio of the maximum velocity (March 4) to the minimum velocity (Sept. 6) 1.29. Now the vernal equinoctial currents determine the magnetic intensity of the southern hemisphere in high latitudes, and the autumnal equinoctial currents that of the northern hemisphere; and the ratio of intensities at the poles (dip 90°) should be nearly equal to that of the maximum and minimum velocities (1.29). According to Gauss's charts, its actual value is 1.32*.

[To be continued.]

* It should have been stated in the text (p. 271), that the rotation of the earth virtually shifts the point of normal impact of the æther to the east of the 6 A.M. meridian, and so delays the morning critical hours.

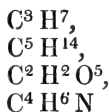
We propose to discuss, very briefly, in the next Number of Silliman's Journal, the remaining topic of our memoir—Chemical Action.

XLIII. *On the Doctrine of Uniform and Constant Saturation.*

By J. ALFRED WANKLYN, Esq.*

WITHIN the last ten years a new feature of great philosophical interest has made its appearance in organic chemistry. It has gradually come to be recognized that the compounds belonging to organic chemistry, notwithstanding their numbers and diversity, have certain points of resemblance in their structure. And so invariable is this resemblance, that the chemist can confidently predict that many forms of structure which may be proposed are incapable of being realized. Obviously a knowledge of that which is essential to the existence of a chemical structure must involve the power of specifying hypothetical forms of structure which, not possessing these characteristics, are impossible structures. And the certainty with which the chemist is able to designate an impossible kind of structure, and the number and variety of the structures which may be marked as impossible, must be excellent signs of the degree in which chemical structure is understood.

From this point of view the progress of chemistry during the last ten years must be regarded as very great; for the chemist is now able to designate as impossible, numbers of structures the occurrence of which ten years ago would hardly have surprised him. Thus, for example, the forms



are recognized as impossible structures; and the list might be indefinitely extended.

In investigating the nature of the principles in virtue of which these and similar forms are pronounced to be impossible, it will at once strike the reader that these examples do not offer electro-chemical difficulties. They are not cases of want of sufficient electro-chemical contrast; and indeed experiment has lent very little support to the otherwise not unreasonable supposition, that groups of atoms too nearly alike in electro-chemical character, or belonging too much to the one or other extreme of the electro-chemical scale, should constitute impossible forms of combination. Hypochlorous acid (Cl^2O) exists, and yet both oxygen and chlorine occupy the negative extreme of the electro-chemical scale. Perchloric acid (HClO^4), although containing so little positive hydrogen to counterbalance so very negative a grouping as ClO^4 , is a compound endowed with considerable stability. And at the extreme positive end of the scale much the same

* Communicated by the Author.

thing is observable ; for there exists a compound containing the very positive potassium linked with the very positive ethyle.

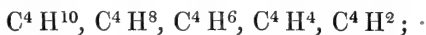
It is not from the side of the electro-chemical theory that the prohibitory statutes have issued, but from the theory of uniform and constant saturation. This theory, which is now taking a very definite form, and which appears to be the highest generalization yet attained to in chemistry, has been drifted into, as it were, during the last ten years. Its nature may be explained as follows. Every atom of which matter is composed has a constant and invariable capacity of saturation (which differs according to the kind of matter) ; and in whatever form of combination the atom happens to be placed, it is always in a state of perfect saturation. Impossible forms of combination are obviously forms in which atoms are represented as not saturated.

Sometimes the atom is saturated with atoms unlike itself, and sometimes it is saturated with atoms like itself. The saturation by a similar kind of atom may be complete or partial ; but when partial, the deficiency must be made up by the presence of other atoms. When two or more similar atoms effect a partial saturation of one another, there results a kind of compound atom—incapable of isolated existence, and requiring the presence of a suitable number of atoms. Complications of structure are produced by partial saturations of this kind. The most complete exemplification of the theory is afforded by the hydrocarbons. The atom of carbon requires four atoms of hydrogen to saturate it. There is only one hydrocarbon known whose structure offers only one atom of carbon in union with hydrogen. This hydrocarbon is marsh-gas, CH^4 . There are multitudes of hydrocarbons besides, but all of them contain carbon-atoms more or less saturated by carbon-atoms ; all of them have a greater condensation of carbon than marsh-gas.

If one atom of carbon be partially saturated by a second atom of carbon, there result complex groups which require either six, four, or two atoms of hydrogen for their saturation. Thus two detached atoms of carbon would take eight atoms of hydrogen and produce two independent chemical structures. If the two atoms of carbon enter into combination to the least possible degree, two atoms of hydrogen must be set free ; if they combine doubly with one another, then four atoms of hydrogen must be evolved ; if triply, then six of hydrogen must go ; and if they perform a total saturation, all eight atoms of hydrogen must leave.

Now, as a matter of experiment, the labours of chemists have produced three and only three hydrocarbons which contain, in the same volume of gas, only twice as much carbon as is present in marsh-gas. They are C^2H^6 , hydride of ethyle ; C^2H^4 ,

olefiant gas; and $C^2 H^2$, acetylene,—just what the theory demands. Passing on to the hydrocarbons of triple condensation, theory indicates the existence of $C^3 H^8$, $C^3 H^6$, $C^3 H^4$, and $C^3 H^2$, and denies the possibility of any other forms. As a matter of fact, chemists know $C^3 H^8$, $C^3 H^6$, and $C^3 H^4$ — $C^3 H^2$ being yet undiscovered; but besides these, no other hydrocarbon containing only C^3 has been found. Of the C^4 series, theory requires



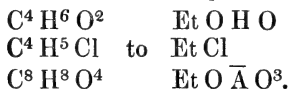
and four of them are known. In short, we know an immense multitude of hydrocarbons of all degrees of condensation, containing in the standard volume C^1 up to C^{30} and more; and there is not a single case which is in opposition to the theory. Whatever the number of atoms of carbon which may be piled up together, the saturating power of the carbon is invariably equal to four atoms of hydrogen.

Nothing is calculated to give a better idea of the scope of the modern doctrine of uniform saturation than a survey of the different phases through which the atomic theory has passed.

As originally propounded by Dalton, and as understood for a number of years and maintained by old stagers even at the present day, the theory was just this:—matter is made up of indivisible atoms which vary in weight according to the kind of matter. The elements are composed of these atoms. When atoms of different kinds link themselves together, there result groups of atoms. Compounds are composed of these groups of atoms. With the nature of the grouping, whether, for example, it should necessarily be a group of two, three, or a dozen atoms that constitute a group, the original atomic theory had no concern. All that was contended for was that the group should not be a very complicated one. The basis of experiment which was represented by this crude state of the theory was this fact. If we take the analysis of any one of the common inorganic compounds, and instead of writing the quantity of each element contained in a hundred parts, divide the quantity of each element by a certain factor varying for each element, we obtain a comparatively simple numerical expression, or at any rate an expression which is easily converted into a very simple one. The factor then became the atomic weight: the simple numerical expression which, when reduced, was often as simple as 1:1, or 1:2, or 2:3, &c., expressed the numbers of the different atoms composing the group. When the atomic theory came to be applied to the compounds belonging to organic chemistry, it was found that the numerical expression arrived at by this process was anything but simple, being now and then quite as complicated as the percentage statement of the analysis. In presence of this difficulty,

chemists evolved the theory of compound radicals and the theory of types. The former consisted of a kind of reduplication of the atomic theory, a compound radical being an atom built up of atoms.

Compound radicals reduced the expressions

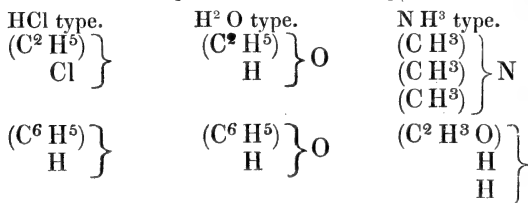


It will be observed that the language of compound radicals expressed the fact that compounds containing certain subordinate groups may be operated upon and changed in a variety of ways without disturbance of the interior arrangement of the subordinate group.

The type was a structure which might be very complicated, but which expressed a variety of different compounds according to the amount of substitution which it underwent. Both theories were embodiments of the uniformities which had been recognized between the structures of different compounds.

But under the theory of compound radicals, the radicals themselves were regarded as ultimate facts, no attempt being made to deduce their nature and constitution from definite fundamental properties of the elementary atoms of which they were built up. In like manner the type was an independent thing, and no limit was hinted at of the numbers or varieties of either radical or type. Although both radical and type were thus made points of departure, the employment of them, and the habits of thought thereby induced, tended continually to the idea of deducing the form of structure of organic compounds from fundamental properties of the elementary atoms.

Rather more than ten years ago two important steps were taken in this direction; and to these two attempts we owe the doctrine of uniform and constant saturation such as we have it to-day. The most celebrated of these was Gerhardt's scheme of including all organic compounds under three great types; viz. Hydrochloric acid, Water, and Ammonia. It was a sort of fusion of the compound-radical with the type theory, organic bodies being brought under these types by the assumption that they contain suitable compound radicals; *e. g.*,



Although the Gerhardt type theory is a most palpable perversion of fact, for organic compounds fall quite as naturally under three dozen general types as under these favoured three, and although it has been abandoned with surprising rapidity, and although, like the simple compound-radical theory of Berzelius and Liebig, it simply accepts a multitude of radicals without offering any explanation of their structure, yet it has completely changed the aspect of the science. It was put forward in a most splendid manner, and brought into prominence many of the most fundamental doctrines,—*e. g.*, that the elements are not composed of isolated atoms, but of groups of similar atoms; that chemical change consists in the vast majority of cases in double decomposition; that the occupation of equal volumes in the state of vapour is an excellent sign of the relative magnitudes of chemical groups; and much besides which I have no time to point out.

The other attempt was that of Kolbe and Frankland, who more than ten years ago had made the practical discovery of the real saturating capacity of carbon, and who recognized that by simple double decomposition the homologous series of alcohols and acids might be built up. It is a remarkable fact, that although Kolbe and Frankland were actually employing methods the validity of which depended upon the fact that the saturating power of the atom of carbon is equal to four of hydrogen, it should have been reserved for Kekulé to make the announcement that if we assign to carbon a fundamental saturating power equal that of four atoms of hydrogen, we are able to exhibit organic compounds with the form of their structure deducible from the fundamental saturating power of the carbon which they contain.

XLIV. *Note on the Periodical Changes of Orbit, under certain circumstances, of a particle acted on by a central force, and on Vectorial Coordinates, &c., together with a new Theory of the Analogues to the Cartesian Ovals in Space, being a Sequel to "Astronomical Prolusions."* By Prof. J. J. SYLVESTER, F.R.S.*

A VERY singular and previously unnoticed species of discontinuity arises when, according to the equations of motion interpreted in the ordinary manner, a particle solicited by a continuous central force *would seem* as if it ought to describe an orbit external to the force-centre. An instance of this kind, probably for the first time, presented itself in a question incidentally brought forward by myself in the paper inserted in the

* Communicated by the Author.

January Number of this Magazine, where I alluded, in passing, to the case of a body acted on by a central force capable of making it move in a circle exterior to the force-centre, and fell into the not unnatural error, which has since been pointed out to me, and which is obvious on a moment's reflection, of stating that on arriving at a point where the motion points to the force-centre, *i. e.* at the point where the tangent to the circle passes through this centre, the particle would go off in a straight line on account of the motion and the force coinciding in direction. But it is clear

that since the instantaneous area $\frac{1}{2}\rho^2 \frac{d\theta}{dt}$ remains finite at such point,

it cannot abruptly become zero; the radial velocity becoming infinite, does not entitle us to reject the transverse part which remains finite; thus the radius vector ρ will continue to revolve in the same direction as before it reached the tangential point; it will therefore swing off to another curve, so that the true orbit will possess an inflexion at that point. The new curve, it may easily be proved, will be a circle equal to the former, and related to it in the manner following: let us suppose O to be the force-centre and two tangents drawn from O to meet the original circle in A and B, so that the line A B divides the circle into two unequal segments, and that the particle has been travelling, say in the upper segment, from A to B; draw the angle B O C equal to the angle A O B, and in it place a circle equal to the former, touching the part O B, O C in B and C; then the particle will describe the *lower* segment of this new circle; and so in like manner, after reaching C, will undergo a new inflexion at that point and pass on to a new circle touching O C and O D, the latter inclined to the former at the same angle as O C to O B and O B to O A. Thus, if we repeat the angular sector A O B indefinitely, and in each such sector place equal circles touching the rays of the sector, and call their upper and lower segments P, Q respectively, the particle will describe the successive arcs $P_1, Q_2, P_2, Q_3, P_3, \dots ad\ infinitum$. If the sectorial angle be an even aliquot part of 360° , the complete orbit will be a single anautotomic broken curve returning into itself, as, for instance, if the sector be 90° the orbit will be $P_1, Q_2, P_2, Q_2, P_3, Q_3, P_4, Q_4, P_1, Q_2, \dots$. If the angle be an odd aliquot part of the same, the orbit will be a line returning into but crossing itself as many times as there are circles, so that in fact the whole of each circle will be described in a complete period, *viz.* the upper and lower segments alternately in the first half period, and the lower and upper in the second half thereof, the *period* being double the time of a revolution if the latter is defined as the interval between the body leaving and returning to any the same point.

Thus, *ex. gr.*, if the angular sector be 72° , the orbit will be

$P_1 Q_2 P_2 Q_3 P_3 Q_4 P_4 Q_5 P_5 Q_1 P_2 Q_2 P_3 Q_3 P_4 Q_4 P_5 Q_5 P_1$ &c.

In like manner, if the angle included between the tangents be any commensurable part of 360° , as $\frac{m}{n} 360^\circ$, where m and n are integers prime to one another, the orbit will be a closed one containing mn alternate segments, or mn entire circles, according as n is even or odd. By taking n even and giving m any arbitrary odd value, a waving line will be produced forming an original and, I think, elegant pattern for a *circular lace border*. For this purpose $\frac{m}{n}$ should not be too small, in order that the disproportion between the alternate circular segments and the ratio of the border to the interior may not be so great as to offend the eye; and m not too great, in order that the traces of the pattern may not become too complicated.

I conjecture that [$m=3$; $n=64$] and [$m=5$; $n=128$], producing respectively 3 and 5 twists, and 5 or 6 and 6 or 7 flexures within a quadrant of each twist, would be eligible systems for the purpose. In general n ought to be even, and m a large moderate odd integer.

If the angle between the tangents to the circle from the force-centre be not an aliquot or commensurable part of 360° , the orbit will be a non-reentrant curve intersecting itself an infinite number of times. Similar or analogous conclusions are of course applicable to every case where the orbit, *seemingly* indicated by the equations of motion, is an oval, or, more generally, any curve to which tangents admit of being drawn from the force-centre,—a self-evident (now that it is stated) but none the less a very surprising feature in the *mathematical* theory of central forces. I say *mathematical*; for it ought in fairness to be observed that since it is impossible to conceive a force of infinite magnitude resulting from the attraction of a finite mass, the question involves not so much a discussion of any real phenomenon, as of the principles of interpretation applicable to an extreme case, valueless as to the establishment of a distinct independent conclusion, although not without latent importance as a safeguard against errors which might flow from the adoption of an erroneous mode of interpretation*.

* If we accept the very reasonable axiom that no law of force is admissible which would involve the consequence of a *finite* mass exerting an infinite attraction at a finite distance, we can find an *à priori* limit to the negative exponent of the power of the distance which can *possibly* express any law of force in nature. If my memory serves me aright, a distinguished rising French analyst, in contravention of this axiom, has assumed, for the

It may readily be found that the velocity at any point of the orbit must be that due to infinity (otherwise a different and much more complicated curve would result), and then with the usual notation the differential polar equation to the curve becomes

$$\left(\frac{d\theta}{dr}\right)^2 = C \frac{(r^2 - k^2)^2}{\sqrt{r^4 - r^2(r^2 - k^2)^2}},$$

which is easily seen to be true of any arc of a circle. The phenomenon to be noticed is, that when $\frac{d\theta}{dr} = 0$, since t is the real independent variable, θ does not attain a maximum or minimum, for it is $\frac{dr}{dt}$ becoming infinite, not $\frac{d\theta}{dt}$ becoming zero, which accounts for $\frac{d\theta}{dr}$ vanishing; accordingly $\frac{d\theta}{dr}$ in passing through zero must be taken with a change of sign, which accounts for the discontinuity of the orbit regarded as a geometrical curve. This change of sign in the radical is very analogous to what happens when we calculate the potential of a spherical shell, and trace its value as the attracted point continuously receding from the centre passes from within to without the shell.

As connected with this subject of motion in a circle, I may mention that Mr. Crofton has pointed out to me that my theorem concerning a homogeneous circular plate whose molecules attract according to the inverse fifth power of the distance, namely that its resultant attraction is capable of making a particle move in any circle cutting the plate orthogonally, admits of being established upon my own principles without calculating, as I have done, the law of the attraction (*Astronomical Prolusions*, Phil. Mag. Jan. 1866, p. 73); for the whole plate may be shown to be its own inverse in respect to any such orthogonal dividing circle; *i. e.* the two parts into which it is divided by the plate will be inverses to each other in respect to the orthogonal circle, and consequently conjointly will serve to make a particle move in a segment of such circle exterior to the plate*.

purpose of explaining certain optical phenomena, a law of force according to some very high inverse power of the distance transcending such limit. It will be seen below that the inverse fifth power is inadmissible on this ground, and is capable of leading to irreconcilable contradictions.

* And equally it follows that a homogeneous plate whose molecules exert a *repulsive* force following the inverse fifth power of the distance, would serve to make a particle move in the *interior* segment of an orthogonal circle. Quære as to how the motion must be conceived to take place when the attracted or repelled particle enters or quits the plate? To fix the ideas, suppose the plate attractive. The orbit described within the plate must

Mr. Crofton has also made a partial extension of the theorem to the case of a plate of the form of either one of a conjugate pair of Cartesian ovals, in a remarkable paper on the theory of these curves, lately read before the London Mathematical Society. In the "Prolusions" I raised the question of determining the force at a focus required to make a body move in such oval. This may easily be solved by aid of vectorial coordinates; and as it seems desirable to place on record the tangential affections of a curve expressed in terms of such coordinates, which I am not aware has hitherto been done, I subjoin the investigation for the purpose. The results will be seen to be of great use in simplifying the solution of the important problem of determining the most general motion of a body attracted to two or more fixed centres, a problem to which I purpose hereafter to return.

If F, G be two foci, c their distance from one another, f, g from any point in a curve, ds the element of the arc at the point (f, g) , θ, η the angles which ds makes with f and g , we have

$$\cos \theta = \frac{df}{ds}, \quad \cos \eta = \frac{dg}{ds}.$$

Call $g+f=u$, $g-f=v$, and let ω be the angle between f and g . Then

$$(\cos \theta)^2 + (\cos \eta)^2 + (\cos \omega^2) - 2 \cos \omega \cdot \cos \theta \cdot \cos \eta - 1 = 0^*.$$

touch the radius, for the force becomes infinite in the direction of the radius, and must tend towards the centre without becoming convex to it, on account of the force being attractive. I do not see how these conditions can be reconciled, except by supposing the remainder of the motion to take place along the radius itself, which involves the supposition of the transverse velocity at immersion becoming instantaneously destroyed, and the same at emergence when the force is repulsive.

* The left-hand side of this equation, calling the directions of f, g, ds , A, B, C , is

$$\begin{vmatrix} 0 & \cos AB & \cos AC & 1 \\ \cos BA & 0 & \cos BC & 1 \\ \cos CA & \cos CB & 0 & 1 \\ 1 & 1 & 1 & 0 \end{vmatrix}$$

and in like manner for four lines in space A, B, C, D in spaces, the determinant

$$\begin{vmatrix} 0 & \cos AB & \cos AC & \cos AD & 1 \\ \cos BA & 0 & \cos BC & \cos BD & 1 \\ \cos CA & \cos CB & 0 & \cos CD & 1 \\ \cos DA & \cos DB & \cos DC & 0 & 1 \\ 1 & 1 & 1 & 1 & 1 \end{vmatrix} = 0.$$

This important equation is nowhere *explicitly* given in treatises on trigonometry or determinants, but is virtually included in a theorem which is to

Hence

$$(ds)^2 = \frac{df^2 + dg^2 - 2 \cos \omega \cdot df \cdot dg}{(\sin \omega)^2}.$$

Let

$$g + f = u, \quad g - f = v.$$

Then, by trigonometry,

$$1 + \cos \omega = \frac{u^2 - c^2}{2fg}; \quad 1 - \cos \omega = \frac{c^2 - v^2}{2fg}.$$

Hence

$$\begin{aligned} (ds)^2 &= \frac{(du)^2(1 - \cos \omega) - (dv)^2(1 + \cos \omega)}{(\sin \omega)^2} \\ &= \frac{(c^2 - v^2)du^2 + (c^2 - u^2)dv^2}{(u^2 - v^2)}; \end{aligned}$$

and again,

$$\begin{aligned} (\sin \theta) &= \sqrt{\frac{(ds)^2 - (df)^2}{(ds)^2}} = \frac{df - dg \cdot \cos \omega}{\sin \omega ds} = \frac{du(1 - \cos \omega) - dv(1 + \cos \omega)}{2 \sin \omega ds} \\ &= \frac{(c^2 - v^2)du - (u^2 - c^2)dv}{4fg \sin \omega ds} \\ &= \frac{(c^2 - v^2)du + (u^2 - c^2)dv}{4fg \sin \omega ds} = \frac{(c^2 - v^2)du - (c^2 - u^2)dv}{\sqrt{(u^2 - v^2)((c^2 - v^2)du^2 + (c^2 - u^2)dv^2)}}, \end{aligned}$$

and similarly,

$$\sin \eta = \frac{(c^2 - v^2)du + (c^2 - u^2)dv}{\sqrt{(u^2 - v^2)((c^2 - v^2)du^2 + (c^2 - u^2)dv^2)}}.$$

It is worthy of passing observation that the above expressions lead immediately to the integral of the fundamental equation in the addition of elliptic functions; for if we call p , q the two perpendiculars from the foci upon ds , we have

$$\frac{(c^2 - v^2)^2(du)^2 - (u^2 - c^2)^2(dv)^2}{((c^2 - v^2)du^2 - (u^2 - c^2)dv^2)} = 4fg \sin \theta \cdot \sin \eta = 4pq.$$

Suppose now

$$4pq = c^2 - a^2.$$

be found in Balzer, and probably elsewhere, as affirmed concerning the four sides of a wry quadrilateral; for *any* four lines in space which meet in a point being given, a wry quadrilateral may be formed with sides parallel respectively to the same. The above equation enables us to express the element of a curve in space in terms of vectorial coordinates and their differentials.

Then

$$(c^2 - v^2)^2 du^2 - (u^2 - c^2)^2 dv^2 = (c^2 - a^2)(c^2 - v^2) du^2 - (c^2 - a^2)(u^2 - c^2) dv^2,$$

or

$$\frac{du^2}{(u^2 - a^2)(u^2 - c^2)} - \frac{(dv)^2}{(v^2 - a^2)(v^2 - c^2)} = 0.$$

The integral, therefore, of this equation must express the fact that u and v are, or may be regarded as, the sum and difference of the distances of two fixed points distant c apart from any point in a fixed straight line, the product of whose distances from those points is $c^2 - a^2$, or also, if we please, as the sum and difference of the distances of two fixed points distant a apart from any point in a fixed straight line the product of whose distances from the points is $a^2 - c^2$.

Thus, parting from the first construction, if we write $y + \lambda x = L$ as the equation to the straight line, the origin being taken midway between the two points, and the axis of x coincident with the line joining them, we obtain

$$c^2 - a^2 = \frac{L^2 - \lambda^2 \frac{c^2}{4}}{1 + \lambda^2},$$

or

$$L^2 = \frac{c^2}{4} \lambda^2 + (c^2 - a^2)(1 + \lambda^2);$$

we have also

$$u^2 = y^2 + \frac{c^2}{4} - cx + x^2; \quad v^2 = y^2 + \frac{c^2}{4} + cx + x^2;$$

so that

$$x = \frac{v^2 - u^2}{2c}, \quad y^2 = u^2 - v^2 - \frac{c^2}{2} - \frac{(v^2 - u^2)^2}{2c^2},$$

and that the required integral will be

$$\begin{aligned} & \sqrt{(u^2 - v^2) - \frac{c^2}{2} - \frac{(v^2 - u^2)^2}{2c^2}} + \lambda \frac{v^2 - u^2}{2c} \\ & + \sqrt{\frac{c^2}{4} \lambda^2 + (c^2 - a^2)(1 + \lambda^2)} = 0, \end{aligned}$$

which, completely rationalized, will lead to an equation of the eighth degree in u , v , and quadratic in λ^2 .

A similar rational equation in u , v , μ^2 can be obtained by interchanging a and c with one another, and λ with μ ; and as each equation represents the complete integral, μ^2 will necessarily

be a linear function of λ^2 when each is regarded as a function of u, v . This linear relation we can establish *à priori*; for we have

$$y + \lambda x = \sqrt{\frac{c^2}{4} \lambda^2 + (c^2 - a^2)(1 + \lambda^2)},$$

$$y + \mu x = \sqrt{\frac{a^2}{4} \mu^2 + (a^2 - c^2)(1 + \mu^2)}.$$

Hence making $x=0$, we have

$$(5a^2 - 4c^2)\mu^2 - (5c^2 - 4a^2)\lambda^2 + 8(a^2 - c^2) = 0.$$

If we are content to leave the integral irrational in λ or μ respectively, then it presents itself under the form of a biquadratic rational equation in u and v . Combining the above construction of the integral with the well-known one through spherical triangles, we obtain an interesting geometrical theorem, viz. that if from a given spherical lune two arcs be cut off by an arc of constant length, their *sines* may always be represented by the sum and difference of the distances of two fixed points from a variable point in a fixed straight line; and moreover there will be two systems of such line and associated points.

Besides the general integral, we have also the singular ones given by

$$u = a \text{ or } v = a, \quad \text{or } u = c \text{ or } v = c,$$

indicating the familiar proposition that the product of the focal distances from the tangents of an ellipse or hyperbola are constant; $u=a$ and $v=c$ will correspond to an ellipse and hyperbola, of which the foci in the one are the vertices of the other, and *vice versa*. If from any external point we draw a pair of

tangents to either of these curves, $\frac{du}{dv}$, i. e. $\frac{df+dg}{df-dg}$, and therefore $\frac{df}{dg}$, will have the same value at each point of contact; so that if

α, α' and β, β' be the angles which the tangents respectively make with the focal distances of the points of contact, we have

$$\frac{\cos \alpha}{\cos \alpha'} = \frac{\cos \beta}{\cos \beta'}, \text{ and also } \alpha' - \alpha \text{ the same in absolute magnitude}$$

as $\beta' - \beta$, from which it is easy to infer $\alpha = \beta, \alpha' = \beta'$, showing that the tangents to an ellipse or hyperbola make equal angles with the focal distances at the points of contact, as is also known from the theory of confocal conics.

In precisely the same manner we may integrate the general equation $F(2p, 2q) = C$, where

$$2p = \sqrt{\frac{u+v}{u-v}} \frac{(c^2-v^2)du + (c^2-u^2)dv}{\sqrt{(c^2-v^2)du^2 + (c^2-u^2)dv^2}},$$

$$2q = \sqrt{\frac{u-v}{u+v}} \frac{(c^2-v^2)du - (c^2-u^2)dv}{\sqrt{(c^2-v^2)du^2 + (c^2-u^2)dv^2}},$$

F being any form of function whatever; the integral will always be

$$\sqrt{(u^2-v^2) - \frac{c^2}{2} - \left(\frac{v^2-u^2}{2c^2}\right)^2} + \lambda \frac{v^2-u^2}{2c} + L = 0,$$

where the relation between L and λ depends upon, and may be determined from, the nature of F*.

As regards the expression for (ρ), the radius of curvature in terms of vectorial coordinates, we may employ the well-known formula

$$\frac{1}{\rho} = \frac{ds^2}{\sqrt{(d^2x)^2 + (d^2y)^2}},$$

where

$$x = \frac{f^2 + c^2 - g^2}{2c} = \frac{uv + c^2}{2c},$$

$$y = \frac{2\sqrt{\frac{u+c}{2} \cdot \frac{u-c}{2} \cdot \frac{c+v}{2} \cdot \frac{c-v}{2}}}{c} = \frac{\sqrt{(u^2-c^2)(c^2-v^2)}}{2c};$$

so that

$$\begin{aligned} 2\frac{c}{\rho} &= \frac{ds^2}{\sqrt{(d^2(uv))^2 - (d^2(\sqrt{(c^2-u^2)(c^2-v^2)}))^2}} \\ &= \frac{(c^2-v^2)du^2 + (c^2-u^2)dv^2}{(u^2-v^2)\sqrt{(d^2(uv))^2 - (d^2(\sqrt{(c^2-u^2)(c^2-v^2)}))^2}}, \end{aligned}$$

which I have not thought it necessary to reduce further. As regards the original question of determining the central force towards a focus, say F, proper to make a body move in a Car-

* By varying the curve to which ds refers, we may obtain innumerable classes of differential equations whose integrals can be determined. Moreover, by taking ds the element of a curve in space referred to three foci, ds can be expressed by aid of the theorem given in a previous footnote as a function of the three focal distances f, g, h and their differentials; and consequently the lengths of the perpendiculars upon it from the three foci can be expressed in like manner, and we may thus obtain integrable forms of simultaneous *binary* systems of differential equations between f, g, h .

tesian oval, we have

$$-F = \frac{1}{2} \frac{d}{df} v^2 = 2h^2 \frac{d}{df} \cdot \left(\frac{1}{2p}\right)^2,$$

where $\frac{h}{2}$ is the instantaneous area, and, if the equation to the oval

be $f - kg = m$,

$$df = kdg; \quad du = (1+k)dg; \quad dv = (1-k)dg; \quad u = \left(1 + \frac{1}{k}\right)f - \frac{m}{k};$$

$$v = \left(1 - \frac{1}{k}\right)f + \frac{m}{k};$$

so that

$$\left(\frac{1}{2p}\right)^2 = \frac{(u-v)((1+k)^2(c^2-v^2) + (1-k)^2(c^2-u^2))}{(u+v)((1+k)(c^2-v^2) + (1-k)(c^2-u^2))^2},$$

from which F may be calculated and expressed under the form

$\frac{P}{f^2Q}$, where P and Q are each rational integral functions of the fourth degree in f .

It does not seem to me worth while to work out the actual values of P , Q for the general form of the oval (in algebra as in common life, there is wisdom in knowing where to stop); but it did appear to me desirable to ascertain the *form* of the expression for the retaining force, which, it is hardly necessary to add, it would have been quite impossible to do had the ordinary systems of coordinates been employed. The fact of this force being a rational function of the distance is a result not without interest; and for particular varieties of the curves belonging to the class of Cartesian ovals, it will be easy to obtain its actual value as a function of the distance.

Postscript.

On the Curve in Space which is the Analogue to the Cartesian Ovals in plano.

By a Cartesoid we may understand a surface such that a linear relation exists between the distances of any point in it from three fixed points in a plane, and by a twisted Cartesian the intersection of two Cartesoids whose three fixed points of reference are identical. A twisted Cartesian, then, will be a curve in space whose distances from three fixed points (its foci) are connected by two linear relations: from this it is obvious that it may be conceived also as the intersection of two surfaces of revolution generated by the rotation about their lines of foci of two plane Cartesians having one focus in common, so that it will consist of a system of closed rings. If F , G , H , K be any four points in a plane,

and if the areas of the triangles GHK, HKF, KFG, FGH be called F_1, G_1, H_1, K_1 respectively, and P be any point in space, it is easy to prove that

$$F_1 \cdot PF^2 - G_1 \cdot PG^2 + H_1 \cdot PH^2 - K_1 \cdot PK^2 = E,$$

where E is a sort of *geometrical invariant* independent of the position of P. Its value may be expressed by the equation

$$-16E^2 = \begin{vmatrix} 0 & FG^2 & FH^2 & FK^2 \\ GF^2 & 0 & GH^2 & GK^2 \\ HF^2 & HG^2 & 0 & HK^2 \\ KF^2 & KG^2 & KH^2 & 0 \end{vmatrix}$$

By making P coincide with F we find

$$\pm E = FG^2 \cdot HKF + FH^2 \cdot GKF - FK^2 \cdot GFH.$$

Hence, if the position of K be determined by linear coordinates, x, y , and of F, G, H by coordinates of the like kind, it is obvious that E becomes a rational quadratic function of x, y ; F_1, G_1, H_1 linear functions of x, y ; and K_1 independent of x, y .

Let P be any point in a twisted Cartesian whose foci are F, G, H; ρ, σ, τ the distances of P from these foci. Then we have

$$l\rho + m\sigma + n\tau + p = 0, \quad . \quad . \quad . \quad . \quad (1)$$

$$l'\rho + m'\sigma + n'\tau + p' = 0, \quad . \quad . \quad . \quad . \quad (2)$$

where $l, m, n, p; l', m', n', p'$ are constants.

Let v be the distance of P from K, then

$$F_1\rho^2 - G_1\sigma^2 + H_1\tau^2 - E = -K_1v^2, \quad . \quad . \quad . \quad (3)$$

and v will be a linear function of ρ, σ, τ , provided that the values of ρ, σ in terms of τ determined from (1) and (2) make the left-hand side of (3) a perfect square.

The condition that this may happen is

$$\begin{vmatrix} F_1; & 0; & 0; & 0; & l; & l' \\ 0; & -G_1; & 0; & 0; & m; & m' \\ 0; & 0; & H_1; & 0; & n; & n' \\ 0; & 0; & 0; & -E; & p; & p' \\ l; & m; & n; & p; & 0; & 0 \\ l'; & m'; & n'; & p'; & 0; & 0 \end{vmatrix} = 0. \quad . \quad (4)$$

It is easy to see that the determinant above written consists exclusively of terms in which only *binary* combinations of F_1, G_1, H_1, E appear. Consequently equation (4) is an equation of the third degree in x, y . When this equation is satisfied, K is a focus just like F, G, H. Hence we may conclude that any

given twisted Cartesian possesses an infinite number of foci, every point that lies in a certain curve of the third degree being a focus. When three foci are given there are four disposable parameters, and no more, for determining this curve, which therefore cannot be any cubic curve, but is subject to satisfy two conditions. This cubic curve of foci for the twisted Cartesian is the analogue of the three focal points appertaining to the ordinary plane Cartesian*.

We are now in a position to obtain a much simpler mode of genesis of the twisted Cartesian. If F, G, H be any three points in a right line whose distances from each of a group of points in a plane more than *two* in number are subject to two linear relations, it is easy to prove that these latter will lie in a Cartesian oval, of which F, G, H are the three foci. If then we draw any transversal in the plane of the focal cubic cutting it in three points F, G, H , and make a plane revolve about this line, each group of points in which the twisted curve is cut by this revolving plane being subject to the same two linear conditions of distance from F, G, H , they and therefore the entire twisted curve will lie in a surface generated by the revolution of a certain Cartesian oval about F, G, H . By drawing F, G, H parallel to an asymptote†, one of the points, say H , goes off to infinity, and F, G become the foci of a conic; and as we may draw any other transversal parallel to the former cutting the cubic in two other points F', G' , we learn that the twisted Cartesian is always expressible as the intersection of two surfaces of revolution of the second degree whose axes are parallel, and is thus a curve of only the fourth order. It follows, moreover, that the focal cubic is the locus of the foci of a family of conics in involution whose axes are parallel.

But we may still further simplify the conception of these remarkable analogues to the ovals of Descartes. One of the system of parallels last described will be the asymptote itself meeting the cubic in only one point, so that the revolving conic becomes a parabola; and again, if we draw another transversal parallel to the asymptote and touching the cubic, the two foci

* It is due to Mr. Crofton to state that the idea which has led to the discovery of this property of the twisted Cartesian was suggested by the method employed by that excellent geometer for establishing the existence of the third focus for the plane ovals, as described by him in a remarkable paper on the theory of these curves read before the London Mathematical Society on the 19th instant. It is important to notice that, since the distances of the points in the twisted curve from any one of the original foci are linearly related to those from any other point L , and also from any other point M in the focal cubic, the distances from L and M are themselves linearly related.

† It will presently appear that there is but one real asymptote to the focal cubic.

come together, and the conic becomes a circle. Hence *every twisted Cartesian is the intersection of a sphere and a paraboloid of revolution**.

We are now in a position to turn back upon the focal cubic itself and make it disclose its true nature; for it will be no other than one of the two curves of foci of the system of conics passing through four points which lie in a circle. The axes of such a system always retain their parallelism; and consequently there will be two separately determinable curves of foci—those, namely, which lie in one set of parallel axes, and those which lie in the other. By a general theorem of M. Chasles, the complete curve of foci is of the sixth order, and consequently each of the two in question ought to be, as we learn from the preceding theory it is, a curve of only the third degree†.

The equation of either may easily be found, and is of the form

$$x(x^2 + y^2 + A) + Bx^2 + Cxy + Dy^2 = 0,$$

to which there is only one real asymptote, viz. $x + D = 0$. This, then, is the general equation to the focal cubic to a twisted Cartesian, and shows it to belong to the class of circular cubics.

The focal cubic is or may be determined by a circle involving three constants and four points arbitrarily chosen in the circle, which, together with the three constants for fixing the plane of the circle, give ten parameters in all.

It passes through the intersections of the three pairs of opposite sides of the quadrilateral inscribed in the circle, the centre of the circle, and the two circular points at infinity; the special relations of the three intersections to the cubic await further investigation. The twisted cubic with which it is associated may be determined by means of two right cones, each involving six constants; but as the axes must be coplanar and parallel, the number of parameters is reduced from twelve to ten, thus showing that, when the focal curve is given, the associated ovals are determined (in this respect differing from the plane ovals, in which one parameter remains indeterminate when the trifocal system of points—the analogue of the focal cubic—is given). It will probably be found that when five points in the focal curve are given, thus leaving two parameters disposable, the twisted ovals drawn through any given point will cut each other orthogonally, as Mr. Crofton has shown to be the case for the plane curves in

* Or, as is evident from the text, the intersection of two (and therefore also of *three*) right cones with parallel axes whose plane will contain the focal cubic.

† Every focal cubic to a given twisted Cartesian has thus its conjugate corresponding to another twisted Cartesian, which may be regarded as the conjugate of the first; and the mutual relations of such curves seem to invite further investigation.

his beautiful paper on the Cartesian ovals. I find that when the focal cubic is defined by means of the circle $x^2 + y^2 - c^2 = 0$, and of its intersection with the parabola $Ax^2 + 2ex + 2fy + g = 0$, its equation becomes $Aex(x^2 + y^2 + c^2) + (A^2 - Ag)x^2 - (ey - fz)^2 = 0$.

I have already implicitly alluded in a preceding footnote, but think it well again to call express attention, to the remarkable property of the new ovals, of giving *circular* perspective projections on the same plane for three different positions of the eye, the lines joining the eye with the centre of each projection being all three parallel to one another and perpendicular to the plane of the picture. This fact involves the truth of the elegant and probably well-known elementary geometrical proposition, that if the opposite sides of a quadrilateral inscribed in a circle be produced, the lines which bisect the acute angles thus formed will be perpendicular to one another, and respectively parallel to the two bisectors of the angles formed by the diagonals at their intersection. I must now leave to professed geometers (among whose glorious ranks I do not claim to be numbered) the further study of those wonderful twin beings, twisted Cartesians as I have called them, but which those who so think fit may of course designate more simply as ovals with the name of their originator prefixed. By supposing the vertices of the three containing cones to be brought indefinitely near to the plane of the picture, my ovals ought to revert to the Cartesian form.

Woolwich Common,
March 26, 1866.

Errata in No. 206.

Page 60, $\pm \cos i = \cos \mu \cos \phi$, for $\cos \mu$ lege $\cos \lambda$.
 $s > \rho_1 + \rho_2$ lege $s < \rho_1 + \rho_2$.

— 67, for $ae = \frac{3a}{5}$ lege $ae = \frac{3a}{5}$.

— 70, footnote, for M. Serret (*bis*) lege M. Ossian Bonnet;
and for centre of force ending singly lege centre of
force acting singly.

— 71, for $\frac{E}{E'} = \frac{r^4}{a^4}$ lege $\frac{E}{E'} = \frac{r^4}{a^4}$.

— 73, for $P = \frac{\mu}{4} \int_0^r dr \int_0^{2\pi}$ lege $P = \frac{g}{4} \int_0^r dr \int_0^{2\pi}$;

and for $P = \frac{\pi\mu}{4} \int_0^r \frac{2(\rho^2 + r^2)dr}{(\rho^2 - r^2)^3}$ lege $P = \frac{\pi g}{4} \int_0^r \frac{2(\rho^2 + r^2)rdr}{(\rho^2 - r^2)^3}$.

— 74, first footnote, for $r'^2 = GO \cdot GF$ lege $r'^2 = GO \cdot GF'$;
and for image circle lege image-making circle.

XLV. *On the Physical Cause of the Submergence and Emergence of the Land during the Glacial Epoch.* By JAMES CROLL*.

IN the Philosophical Magazine for last month Mr. Heath has favoured us with a long and elaborate paper on the glacial-submergence question. In that paper he arrives at the conclusion that an ice-cap placed upon the arctic regions would not only attract the water of the ocean towards those regions, but would also cause a similar flow of water to the south pole, producing a rise in the sea-level there also, the solid nucleus of the globe meanwhile remaining undisturbed in the centre of the ocean. That an ice-cap resting on the solid ground should by its attraction cause the waters of the ocean to recede to the opposite side of the globe, is a conclusion so diametrically opposed to the received principles of mechanics, that we fear there are few physicists who will not be apt to suspect that Mr. Heath in his reasoning must have gone astray somewhere or other. We are inclined to think that he has been misled by adopting an erroneous theory regarding the cause of the tides. Mr. Heath explains the cause of tides as follows:—

“The common explanation,” he says, “of the phenomena of the tides, by reference to the tendency of the ocean, under the influence of the moon or sun, to assume an elliptical shape with the long axis pointing towards the luminary and the solid nucleus of the earth at the centre, would not be in anywise affected by supposing either luminary to be attached to the ‘solid mass of the earth’ by a rod of insensible weight; whereas Mr. Croll’s axiom would lead to the gathering up of the waters, not into two opposite semidiurnal tides respectively under and antipodal to the moon, but into one globular mass under it.”

It is perfectly true that the ordinary phenomena of the tides would occur just as they do at present, though the earth and moon were connected by a “rod of insensible weight,” provided those orbs be permitted to revolve around their common centre of gravity. But stop their motion and allow them to be held separate by the “rod,” which we presume is the idea Mr. Heath wishes to convey, and there would not then be two semidiurnal lunar tides, as he supposes, but only one on the side under the moon, just as my “axiom” would lead us to conclude.

I am of opinion that Mr. Heath has been misled at the very outset by overlooking the fact that the rise of the waters by the attraction of the ice-cap and the rise of the tidal wave are not

* Communicated by the Author.

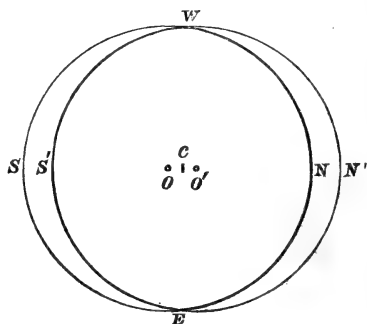
analogous phenomena. The rise of the ocean under the attraction of the ice-cap, as I have already shown*, is a purely statical effect, whereas the tidal wave is as much a kinetic effect as a statical.

He states that the principle on which I have attempted to solve the problem is altogether erroneous. I had expressed the same opinion myself regarding one part of my principle, but in rather too unqualified terms†. From the letter of Professor W. Thomson appended, it will be seen that I had been led to abandon the fundamental plan of calculation used in my original theory somewhat too hastily.

All those who have discussed the subject, with the exception of Archdeacon Pratt, have assumed the earth to be a perfect sphere previous to the ice-cap being placed upon it. I shall, however, assume it to be a sphere after the addition of the ice-cap. This assumption is allowable; for in reality an ice-cap of the form which I shall assume actually makes the earth more spherical than before, for it tends to make the polar diameter more nearly equal to the equatorial. In regard to the form of the cap, each one has chosen that form which appeared to him best adapted to simplify calculations; and of course we are at perfect liberty to do the same. M. Adhémar, who was the first to direct his attention to the subject, and Mr. Heath, and also Mr. Fisher, assume the cap to be of uniform thickness. I shall, however, assume it to be thickest at the pole, and to thin away towards the equator.

We shall begin with a period of glaciation on the southern hemisphere. Let $WNE S'$ be the solid part of the earth, and c its centre of gravity. And let ESW be an ice-cap covering the southern hemisphere. Let us in the first case assume the earth to be of the same density as the cap. The earth with its cap forms now a perfect sphere with its centre of gravity at o ; for $WNE S$ is a circle, and o is its centre. Suppose now the whole to be covered with an ocean a few miles deep. The ocean will assume the spherical form, and will be of uniform depth. Let the southern winter solstice begin

Fig. 1.



* Reader, March 3, 1866.

† Ibid.

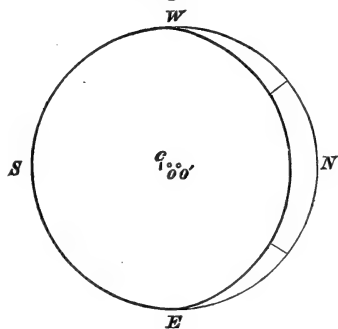
now to move round from the aphelion. The ice-cap will also commence gradually to diminish in thickness, and another cap will begin to make its appearance on the northern hemisphere. As the northern cap may be supposed, for simplicity of calculation, to increase at the same rate that the southern will diminish, the spherical form of the earth will always be maintained. By the time that the northern cap has reached a maximum, the southern cap will have completely disappeared. The circle $WN'ES'$ will now represent the earth with its cap on the northern hemisphere, and o' will be its centre of gravity; for o' is the centre of the circle $WN'ES'$. And as the distance between the centres o and o' is equal to NN' , the thickness of the cap at the pole, therefore NN' will represent the extent to which the centre of gravity has been displaced. It will also represent the extent to which the ocean has risen at the north pole and sunk at the south. This is evident; for as the sphere $WN'ES'$ is the same in all respects as the sphere $WNES$, with the exception only that the cap is on the opposite side, the surface of the ocean at the poles will now be at the same distance from the centre o' as it was from the centre o when the cap covered the southern hemisphere. Hence the distance between o and o' must be equal to the extent of the submergence at the north pole and the emergence at the south. Neglect the attraction of the altering water on the water itself, which later on will come under our consideration.

We shall now consider the result when the earth is taken at its actual density, which is generally believed to be about 5.5. The density of ice being .92, the density of the cap to that of the earth will therefore be as 1 to 6.

Let fig. 2 represent the earth with an ice-cap on the northern hemisphere, whose thickness is, say, 6000 feet at the pole. The centre of gravity

of the earth without the cap is at c . When the cap is on, the centre of gravity is shifted to o , a point a little more than 500 feet to the north of c . Had the cap and the earth been of equal density, the centre of gravity would have been shifted to o' , the centre of the figure, a point situated, of course, 3000 feet to the north of c . Now it is very

Fig. 2.



approximately true that the ocean will tend to adjust itself as a sphere around the centre of gravity o . Thus it would of course

sink at the south pole and rise to the same extent at the north in any opening or channel in the ice allowing the water to enter.

Let the ice-cap be now transferred over to the southern hemisphere, and the condition of things on the two hemispheres will in every particular be reversed. The centre of gravity will then lie to the south of c , or about 1000 feet from its former position. Consequently the transference of the cap from the one hemisphere to the other will produce a total submergence of about 1000 feet.

It is, of course, absurd to suppose that an ice-cap could ever actually reach down to the equator. It is probable that the great ice-cap of the glacial epoch nowhere reached even halfway to the equator. Our cap must therefore terminate at a moderately high latitude. Let it terminate somewhere about the latitude of the north of England, say at latitude 55° . All that we have to do now is simply to imagine our cap, up to that latitude, becoming converted into the fluid state. This would reduce the cap to less than one-half its former mass. But it would not diminish the submergence to anything like that extent. For although the cap would be reduced to less than one-half its former mass, yet its influence in displacing the centre of gravity would not be diminished to that extent. This is evident; for the cap now extending down to only latitude 55° , has its centre of gravity much further removed from the earth's centre of gravity than it had when it extended down to the equator. Consequently it now possesses, in proportion to its mass, a much greater power in displacing the earth's centre of gravity.

There is another fact, referred to above, which must be taken into account. The common centre of gravity of the earth and cap is not exactly the point around which the ocean tends to adjust itself. It adjusts itself not in relation to the centre of gravity of the solid mass alone, but in relation to the common centre of gravity of the entire mass, solid and liquid. Now the water which is pulled over from the one hemisphere to the other by the attraction of the cap will also aid in displacing the centre of gravity. It will cooperate with the cap and carry the true centre of gravity to a point beyond that of the centre of gravity of the earth and cap, and thus increase the effect.

In the 'Reader' for January 13, 1866, I advanced an objection to the submergence theory on the grounds that the lowering of the ocean-level by the evaporation of the water to form the ice-cap, would exceed the submergence resulting from the displacement of the earth's centre of gravity. But, after my letter had gone to press, I found that I had overlooked some important considerations which seem to prove that the objection

had no real foundation. For during a glacial period, say on the northern hemisphere, the entire mass of ice which presently exists on the southern hemisphere would be transferred to the northern, leaving the quantity of liquid water unchanged.

Note on the preceding Paper. By Professor W. THOMSON, F.R.S.

Mr. Croll's estimate of the influence of a cap of ice on the sea-level is very remarkable in its relation to Laplace's celebrated analysis, as being founded on that law of thickness which leads to expressions involving only the first term of the series of "Laplace's functions," or "spherical harmonics." The equation of the level surface, as altered by any given transference of solid matter, is expressed by equating the altered potential function to a constant. This function, when expanded in the series of spherical harmonics, has for its first term the potential due to the whole mass supposed collected at its altered centre of gravity. Hence a spherical surface round the altered centre of gravity is the *first* approximation in Laplace's method of solution for the altered level surface. Mr. Croll has with admirable tact chosen, of all the arbitrary suppositions that may be made foundations for rough estimates of the change of sea-level due to variations in the polar ice-crusts, *the* one which reduces to zero all terms after the first in the harmonic series, and renders that first approximation (which always expresses the *essence* of the result) the whole solution, undisturbed by terms irrelevant to the great physical question.

Mr. Croll, in the preceding paper, has alluded with remarkable clearness to the effect of the change in the distribution of the water in increasing, by its own attraction, the deviation of the level surface above that which is due to the *given* change in the distribution of solid matter. The remark he makes, that it is round the centre of gravity of the altered solid and altered liquid that the altering liquid surface adjusts itself, expresses the essence of Laplace's celebrated demonstration of the stability of the ocean, and suggests the proper elementary solution of the problem to find the true alteration of sea-level produced by a given alteration of the solid. As an assumption leading to a simple calculation, let us suppose the solid earth to rise out of the water in a vast number of small flat-topped islands, each bounded by a perpendicular cliff, and let the proportion of water-area to the whole be equal in all quarters. Let all of these islands in one hemisphere be covered with ice, of thickness according to the law assumed by Mr. Croll—that is, varying in simple proportion to the latitude. Let this ice be removed from the first hemisphere and similarly distributed over the islands of

the second. By working out according to Mr. Croll's directions, it is easily found that the change of sea-level which this will produce will consist in a sinking in the first hemisphere and rising in the second, through heights varying according to the same law (that is, simple proportionality to sines of latitudes), and amounting at each pole to

$$\frac{(1-\omega)it}{1-\omega w},$$

where t denotes the thickness of the ice-crust at the pole; i the ratio of the density of ice, and w that of sea-water to the earth's mean density; and ω the ratio of the area of ocean to the whole surface.

Thus, for instance, if we suppose $\omega = \frac{2}{3}$, and $t = 6000$ feet, and take $\frac{1}{6}$ and $\frac{1}{5\frac{1}{2}}$ as the densities of ice and water respectively, we find for the rise of sea-level at one pole, and depression at the other,

$$\frac{\frac{1}{3} \times \frac{1}{6} \times 6000}{1 - \frac{2}{3} \times \frac{1}{5\frac{1}{2}}},$$

or approximately 380 feet.

It ought to be remarked that a transference of floating ice goes for nothing in changing the sea-level, and that in estimating the effect of grounded icebergs the excess of the mass of ice above that of the water displaced by it is to be reckoned just as if so much ice were laid on the top of an island.

XLVI. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 144.]

BEKETOFF has published a work, in the Russian language, on the displacement of some elements by others. The following account of his principal results is taken from an abstract of the book, which appeared in the *Zeitschrift für Chemie**.

The experiments on the relation of hydrogen under high pressure towards metallic salts are already partially known. The author made an experiment in which he used a tube bent eight times, in such a manner that four legs were formed. In the first bend a dilute solution of *sulphate of silver* was placed, in the second

* June 1865.

a saturated solution, and in the third a saturated solution along with crystals of the same salt, while in the fourth was some sulphuric acid, and in the drawn-out end of the tube some zinc. By inclining the tube, zinc was brought into sulphuric acid; the hydrogen liberated had to pass first of all through the layer of saturated solution of the silver-salt, by which it was purified; it could then act upon the second saturated solution, and finally on the dilute solution. After some days a dark precipitate appeared on the surface of the dilute solution, which increased in the following days, while both saturated solutions remained unchanged. Hence reduction by hydrogen only takes place in dilute solutions.

A dilute solution of *nitrate of copper* was not reduced by hydrogen even under a pressure of forty atmospheres. A mixture of nitrate of copper and silver only gave dubious indications of a separation of copper. Lead solutions could not be reduced by hydrogen.

The author repeated the experiments with the aid of *platinum*. In a glass tube he arranged in layers hydrochloric acid, zinc, and a dilute solution of *copper*, in which dipped a platinum-foil which had been previously ignited. After sealing the tube, the zinc was brought into the hydrochloric acid by shaking. In a few days all the zinc was dissolved, so that the internal pressure must have amounted to about 110 atmospheres. After the lapse of this time a crystalline rose-red deposit could be seen on the platinum.

The author filled glass tubes one-third full with metallic solutions, placed platinum-foils in them so that about half the platinum was out of the solution, and filled the tubes with hydrogen. The tubes were then sealed. On the following day crystallized silver was deposited on the platinum in the solution of *sulphate of silver*. After several days there was a precipitate of copper in the dilute solution of *nitrate of copper*. Lead solutions were unchanged. In these experiments the platinum manifestly acts by its condensation of hydrogen on the surface. It follows from this that hydrogen, as a reducing agent, takes a place between lead and copper, mercury, silver, palladium, gold, and platinum.

In order more accurately to determine the *pressure* under which the reduction of metals takes place, the author repeated his experiments, in tubes one end of which was drawn out. This end was divided, calibrated, and then bent in a U-shape so as to serve as a manometer. Some mercury was placed in the tube; then acid, zinc, and metallic solutions were arranged in layers on one another. After the position of the mercury was read off, the manometer and the other open end of the tube were melted. A solution of 1 part of *sulphate of silver* in 350 parts of water exhibited no decomposition under a pressure of $4\frac{3}{4}$ atmo-

spheres after several days; under 6 atmospheres there was a feeble but distinct reduction of silver; and under 14 atmospheres dark-violet silver was deposited even on the next day. A solution of 1 part of sulphate of silver in 50 parts of water exhibited no decomposition under a pressure of 14 atmospheres, but it did under a pressure of 23 atmospheres.

Experiments with Carbonic Acid.—A 12-per-cent. solution of acetate of lime was enclosed in a tube with marble and hydrochloric acid. When the pressure of the carbonic acid given off amounted to 14·5 atmospheres, no action was perceptible. Under 27·5 atmospheres there was, after a few days, a separation of *carbonate of lime*. In a saturated solution of acetate of lime carbonate of lime was precipitated under a pressure of 28·5 atmospheres. The latter appears in these cases, when seen under the microscope, as globular aggregates of acicular crystals. Inversely, *marble* was sealed up with excess of dilute *acetic acid*. In a few days the pressure rose to 17 atmospheres, but did not further increase even after eight months. Part of the marble remained undissolved. Hence the decomposition of marble by acetic acid stops at pretty much the same temperature as that at which the decomposition of acetate of lime by carbonic acid stops.

A dilute perfectly neutral solution of *chloride of calcium* was not changed under a pressure of 45 atmospheres (the tube in this case contained condensed liquid carbonic acid); nor was *acetate of baryta* under 30 atmospheres, or *chloride of barium* under 60 atmospheres.

The author's experiments on the reducing-power of zinc and aluminium at high temperatures are known from previous communications. Whereas aluminium readily sets free *barium*, it does not reduce *calcium* from oxychloride of calcium. Aluminium can in turn be separated by *magnesium* (from artificial cryolite for instance). If *caustic potash* is heated in a gun-barrel with *aluminium*, globules of potassium are readily obtained. This reaction may perhaps be applicable in practice.

In almost all cases of the displacement of metals by others, the author found *that the element with lower specific gravity expels the one with higher*. This rule of course applies only to elements that are chemically analogous. Such, however, the true metals are, and metallic solutions are therefore well adapted for testing the above rule. From Fischer and Odling's experiments, a Table may be deduced for the metals, in which each element is replaced by each preceding one.

	$d.$	$e.$	$\frac{d}{e} 100.$	$V = \frac{e}{d}$	$\sqrt[3]{\frac{e}{d}} = r.$
K	10.86	39.2	2.2	45.6	3.58
Na	0.97	23	4.2	23.7	2.87
Ca	1.58	20	7.9	12.6	2.33
Mg	1.75	12	14.6	7.0	1.91
Al	2.5	9	27.7	3.6	1.53
—	—	13.5	18.5	5.4	—
Zn	6.9	32.6	21.2	4.6	1.66
Fe	7.8	28.0	27.8	3.6	1.53
Co	8.6	29.5	29.1	3.4	1.51
Cd	8.8	56	15.7	6.5	1.86
Cu	8.9	31.7	28.0	3.6	1.53
—	—	63.4	14.0	7.2	1.93
Pb	11.4	103.5	11.0	9.2	2.09
Hg	13.5	100	13.5	7.3	1.94
—	—	200	6.75	14.6	2.45
Ag	10.4	108	9.6	10.2	2.17
Au	19.3	198	9.7	10.2	2.17
Pt	21.0	98.7	21.2	4.6	1.66

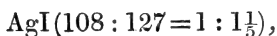
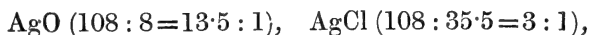
In this case d = spec. grav., e = equivalent.

It is thus seen that almost universally the specifically lighter metal is also the stronger. The further the metals are apart, the more certain is this rule. With elements which are near each other (Sn and Pb, Ag and Hg), the reverse of the rule may be the case. With metals of almost equal specific gravity (Ni and Co, Cd and Cu), the mutual replacement is determined with difficulty. Copper and lead form a surprising exception: although copper is specifically lighter than lead, yet lead, as is well known, easily displaces copper; but copper scarcely displaces lead. Hg and Ag are scarcely to be considered exceptions: mercury displaces silver; but, as Odling found, silver can also displace mercury; and, moreover, mercury is liquid.

Besides the metals, the rule indicated is confirmed by the cases of Cl, Br, and I, and, as far as can be concluded from a few experiments, in the cases of O, S, Se, and Te.

The author discusses the influence of the specific gravity of the elements on their reactions, and thereby reduces chemical affinity to purely mechanical principles.

In chemical decompositions the relative stability of different compounds plays an essential part. The author comes to the conclusion that, *of the compounds of an element, those are most stable in which the equivalents of the elements united are as nearly equal as possible.* Of the silver compounds, for instance,



iodide of silver is the most stable, because here the ratio of the equivalents is nearest unity. Hence it is that AgO so readily decomposes with KI, and in like manner AgCl with KBr and KI. But potash decomposes AgI and AgBr only with difficulty, but AgCl much more easily. The elements have therefore a tendency to form compounds with equal equivalents; and it is not surprising that the author could liberate barium from BaO, but not from BaCl by aluminium.

This, however, forms no exception to the influence of the specific gravity of the elements upon the course of the reaction. According to Wöhler, aluminium can readily liberate Ag from AgI, and the author succeeded in doing the same with manganese. But at a very high temperature aluminium and, especially, magnesium will probably be set free from their respective iodides by silver, as in fact Deville has set potassium free from KI by silver.

The author thinks that all these phenomena, taking into account Dulong and Petit's law, may be deduced from the mechanical theory of heat. He finds a confirmation of his views in the composition of the earth's crust itself. Al and Se, whose equivalents and specific heats are almost equal, are for the most part found combined with oxygen and with one another. Fluorine is mostly combined with Ca, Cl mostly with Na, Ag frequently with I. But the phenomena of the so-called *elective affinity* remain unexplained. If it is intelligible why, for instance, AgO decomposes with KI, it is also inexplicable why K unites with Cl, Br, and even I, but not Ag or Zn.

The compounds which chemically dissimilar elements form are stable, and take place in few ratios; while the compounds of analogous elements (for instance Cl and Br) can take place in several ratios, and have but little stability. In the first case the combination always takes place with more or less disengagement of heat, in the second case seldom or never. If the heat is transmuted into motion, the conclusion is obtained that the elementary motions of one kind of elements (K, Na, &c.) interfere with the motions of the other elements, but not with those of each other. The reason of this peculiar deportment of the elements could only be deduced from hypotheses, upon which the author will not at present enter.

In the thermal phenomena of chemical reactions, the quantity of heat which is used in a change of volume of the compounds must be taken into account. The quantity of heat which becomes free in the union of two bodies is $T = X + B$, in which X is the heat which becomes free in the combination or neutralization of properties, and B is the heat used up in the change of volume.

As T is known from experiment, and B may be calculated, X

(that is, the heat which becomes free solely from combination) can be determined; and it is only chemical equivalents thus corrected which are comparable one with another. In calculating B, the author assumes that elements in their compounds assume equal values. We have then

$$B = \frac{V-2v}{2v \cdot d} \cdot C + \frac{V-2v_1}{2v \cdot d_1} \cdot C_1,$$

in which V, v, v₁ are the atomic volumes, d and d₁ the coefficients of expansion, and C and C₁ the specific heats of the elements. In this way the following Table has been calculated:—

	V.	Atomic volume.		T.	B.	T-B.	d and d ₁ .
	Atomic volume of compound.	Of the metal, v.	Of the haloid, v ₁ .	Favre and Silbermann.			Coefficient of cubical expansion.
KCl	37.4	45.4	26.7	+100960	+ 17183	83777	Ag = 0.00005973
AgCl	26	10.35	26.7	+ 34800	- 30651	65451	Br = 0.001264
AgBr	29.6	10.35	26.7	+ 25618	- 41861	77479	I = 0.000856
AgI	42.7	10.35	25.5	+ 18651	-107757	126408	K = 0.000243

By comparing the coefficient of expansion of the metals with their chemical nature, the author comes to the conclusion that within certain groups of metals (Pb, Fe, Ag, and Sn or Zn, Cd, Cu) the coefficient of expansion is inversely proportional to the number of atoms in the unit of volume, that is = $\frac{d}{e}$, or, what is the same thing, directly proportional to the cube of their distances, *i. e.* = r^3 .

Hittorf has published* a series of experiments on phosphorus, the principal results of which are as follows. In order to study the circumstances under which red phosphorus passes into the condition of ordinary phosphorus, purified red phosphorus was hermetically sealed in glass tubes from which the air had been completely exhausted, and then exposed to various temperatures. This was effected by suspending such tubes in test-tubes in which the vapours of various substances were formed, the temperature of which varied from 255° C. to 530° C. The substances, with the corresponding temperature of their vapours, were benzoic acid, 255° C.; chloride of mercury, 307° C.; bromide of mercury, 324° C.; iodide of mercury, 358° C.; sulphur, 447° C.; and sulphide of phosphorus, 530° C. In some cases the arrangement was so modified that the tubes could be heated in the

* Poggendorff's *Annalen*, October 1865.

vapours at different pressures, by which other temperatures were obtained. This mode of heating has two advantages over the use of liquids in producing high temperatures: in the first place, the temperatures are quite uniform, and may be retained for any length of time; in the second, the interior of the exhausted tube is visible, and that, too, in the case of strongly-coloured vapours, such as that of sulphur, provided a flame is held behind the tube.

Red phosphorus, at temperatures of 307° and 324° , became somewhat darker, without any other change; but when heated in the vapour of chloride of mercury (358° C.), a few drops of colourless phosphorus were found in the upper part of the tube, the quantity of which did not increase, provided this temperature was not exceeded. When, however, the vapour of sulphur or of sulphide of phosphorus was used, the quantity of colourless phosphorus materially increased. While the glass tube is heated, this quantity of colourless phosphorus fills the interior of the exhausted tube as vapour; in a given space at a given temperature, a definite quantity of vapour is formed from the red phosphorus without the latter becoming liquid. When this maximum of density and tension is attained, the rest of the phosphorus remains unchanged. On cooling, this vapour condenses as colourless phosphorus. If the quantity of this colourless phosphorus which is formed at a given temperature in a small tube of known capacity is determined, its maximum density is obtained. As the vapour of phosphorus is sixty-two times as heavy as hydrogen at the same temperature and under the same pressure, the maximum tension is obtained from the density found, assuming that Boyle and Gay-Lussac's law holds good. In this way tension of phosphorus-vapour is obtained for the following temperatures:—

Temperatures.	Weight of a litre of the vapour.	Tension in millimetres.
388°	0.0996 gm.	31.5
409	1.0850	370.6
447	4.538	1636.5
530	15.625	6139

The last result is a little too high.

Schrötter adduces 260° C. as the temperature at which red phosphorus changes into colourless phosphorus. Hittorf found that no change took place even with four hours' heating at 255° ; nor did oxygen combine with it at this temperature. Under 260° red phosphorus is not volatile, and from that temperature it gives vapours with continually increasing density and tension.

The vaporization of red phosphorus is exceedingly slow as compared with that of a liquid body.

An exhausted glass tube containing red phosphorus was screwed up in an iron one, the interstices being filled with magnesia pressed down. The tube was then heated in the flame of five Bunsen's burners, the temperature of which was gradually raised. In the cases in which the tubes held out, besides the drops of colourless phosphorus, the red phosphorus was found to be merely sintered together, and not melted. In this respect, as comparative experiments showed, the deportment of phosphorus quite resembles that of arsenic: arsenic heated in a closed tube volatilized and formed beautiful crystals without melting.

Colourless phosphorus, when heated in an hermetically sealed vessel (an iron tube can be used), is changed into red phosphorus by being heated for a short time to a temperature of 300° C.

An attempt was made to determine directly the heat which becomes free when colourless changes into red phosphorus, by placing a thermometer in some phosphorus contained in a flask which was heated in an air-bath. When the temperature of the bath was 295° , the thermometer in the phosphorus marked 282° ; at this point the temperature of the latter began rapidly to rise and soon attained 370° C.; the cork in which the thermometer was placed was not tight at this temperature, and phosphorus-vapours escaped and caused the experiment to be stopped.

Schrötter found the tension of colourless phosphorus to be 760 millims. at 290° ; comparing this with that of red, which at 358° is only 31 millims., and considering that above 300° liquid colourless phosphorus cannot exist, it would appear possible to drive phosphorus from a position of lower temperature to one of higher. An exhausted tube containing ordinary phosphorus was exposed at one end to the temperature 255° , and at the other to that of 358° C.; the hotter part remained, however, quite empty; no red phosphorus was deposited there. It follows from this that colourless phosphorus-vapour can retain a higher tension and density than that which is formed from red phosphorus. A number of special experiments show that when colourless phosphorus was used instead of red, far higher densities and tensions were obtained for the same temperatures.

Colourless phosphorus in the exhausted tubes, when the temperature is raised, has probably passed partially into the vaporous state. The part which has remained liquid passes into the state of red phosphorus, and, by its disengagement of heat, materially increases the temperature of the surrounding medium, and thus gives a higher density and tension to the vapour. Experiment showed that the tension and density which the vapours at first obtain are not stable and continually diminish, while red phos-

phorus is deposited. Only after about two hours' heating is a stable maximum obtained; it very slowly increases with the temperature, and is always considerably greater than that disengaged at the same temperature from red phosphorus.

A large quantity of colourless phosphorus was heated in hydrogen gas under various pressures, which increased to four atmospheres. When heated over free flame it always boiled rapidly; the formation of vapour was always more rapid than the change into red phosphorus.

From Hittorf's experiments it follows that the vapour of red phosphorus may be cooled from 447° to 358° (that is, through 90°) without changing its condition. This was confirmed by the fact, that in an exhausted tube one end of which was heated to 358° and the other, which contained red phosphorus, to 447° , no deposit of phosphorus was observed in the first. In order to observe the pressures directly, phosphorus was placed in the closed end of an exhausted siphon-barometer the bend of which was closed by bismuth; this metal does not combine with phosphorus at 447° , and only absorbs its vapour to a small extent. The open leg was filled with hydrogen and was connected with a manometer, so that, by disengagement of hydrogen and pressure of mercury, pressure could be exerted on the bismuth. When the siphon part was heated to 447° , the phosphorus-vapour had a pressure of $1\frac{1}{4}$ atmosphere, much less, therefore, than 1633 millims., which the first experiments gave; the difference arises from the absorption of phosphorus by the bismuth. It could, however, be shown that, spite of the fact that the formation of vapour had ceased, the vapour had not the density which it could possess at this temperature; for the pressure of the hydrogen could be raised to $3\frac{1}{2}$ atmospheres before the short leg was completely filled with bismuth. If the pressure was again diminished, a perceptible and very slow formation of vapour ensued under a pressure of $1\frac{1}{2}$ atmosphere.

If an exhausted tube containing red phosphorus be heated in one part to 530° , and in another to 447° , microscopic needles are deposited in the cooler part. These crystals were obtained of a larger size by taking advantage of the property which lead has of dissolving phosphorus at high temperatures and depositing it on cooling. Colourless phosphorus was placed with lead in a strong hard glass tube, which was exhausted of air and then sealed. This tube was placed in an iron one which had screw plates at each end, the interstices being filled with magnesia, and the whole heated in the flame of five Bunsen's burners for 8 or 10 hours. The phosphorus was obtained on the surface of the lead as striated prismatic laminae several lines in length and bent like tulip-leaves; they had a metallic lustre, were black in reflected

and red in transmitted light, and unalterable in the air. The lead becomes much less fusible, and that which has been already used is more suited for the formation of crystals on its surface. The lead contains crystals dissolved which are not detected on cutting it, but may be obtained on dissolving the lead by the prolonged action of cold nitric acid of 1.1 spec. grav. The crystals thus obtained appear to be rhombohedra, and are thus probably isomorphous with the crystals of As, Sb, and Bi. There is then a metallic phosphorus which is amorphous in Schrötter's modification and crystallized in this new form, and a non-metallic form—that originally discovered by Brandt. The new metallic phosphorus has the spec. grav. 2.34 at 15°·5 C. ; and its atomic volume, $\frac{31}{2.34} = 13.25$, is identical with that of metallic arsenic.

Amorphous phosphorus, when heated for a long time, passes into the crystalline modification.

Crystallized phosphorus is less volatile even than amorphous. It forms no colourless phosphorus below 358°. The maximum tension and vapour-density are at all temperatures far lower than those of amorphous phosphorus. Thus at 447° a litre of the vapour weighs 2.573 grms. and has the tension 928 millims. At 530° the weight is 10.198 to 10.338 grms., and the tension 4101 to 4158 millims. The author points out that phosphorus is probably the first body which has a different tension in its different modifications. He shows, too, that the vapour of colourless phosphorus retains its density and tension unaltered even if in contact with the amorphous metallic phosphorus.

Hittorf states that Geissler, by placing colourless phosphorus-vapour in closed glass tubes, has changed it into red by passing the electric spark through it while in the state of vapour. The author shows that sunlight does not change the vapour, nor does the electric light. Yet a white heat effects the change to a small extent, as is seen when phosphorus-vapour is driven through a white-hot porcelain tube by means of a current of hydrogen. The same effect is produced if charcoal-points placed in the vapour are raised to incandescence by the electric current.

XLVII. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from p. 233.]

Feb. 26, “ON the Papyrus of the Lake of Gennesaret.” By 1866. Professor Cardale Babington.

The author pointed out the distinctions between *Cyperus papyrus* and *Cyperus syriacus*. The former, the papyrus of history, grows in Nubia and on the White Nile, and was found (by Rev. H. B.

Tristram) on the shores of the Lake of Gennesaret, and in a large bog near Lake Hûleh. The latter grows in Sicily and on the Syrian coast: this is the plant cultivated in storehouses in England.

“On a Specimen of *Echinarachnius Woodii*, from the Coralline Crag.” By Professor Liveing.

The author exhibited a very perfect specimen from near Aldborough. He explained his reasons for differing from the late Professor Edward Forbes, who had named and described the fossil from two imperfect specimens; and he assigned it to the genus *Rhynchopygus*.

“On a New Theory of the Skull and of the Skeleton; with a Catalogue of the Fossil Remains of Vertebrate Animals contained in the Woodwardian Museum.” By Mr. Harry Seeley.

The author endeavoured to prove that growth in bone was the result of forces acting upon it; so that the stimulants of growth were pressure and tension. He argued against the theory which considered the skull to be a development of *three* vertebræ, and pointed out analogies between the several parts of the skull and the epiphyses and centrum of a vertebra. He considered the brain-region to be a modified vertebra, and the bones about the breathing apertures the modified end of the trachea.

March 12.—“On the Homeric Tumuli.” By Mr. Paley.

The author described the funeral rites of the Greeks and Trojans as narrated in the Homeric Poems, and explained the mode in which the tumuli were constructed—pointing out resemblances between these descriptions and the tumuli which have been examined in various parts of Europe.

“On the Method of demonstrating some Propositions in Dynamics.” By Mr. Todhunter.

ROYAL SOCIETY.

[Continued from p. 237.]

February 1, 1866.—Lieut-General Sabine, President, in the Chair.

The following communications were read :—

“On the Specific Gravity of Mercury.” By Balfour Stewart, M.A., LL.D., F.R.S., Superintendent of the Kew Observatory.

Some time since, in connexion with a research on the fusing-point of mercury, several observations were made at Kew of the specific gravity of this fluid.

A specific-gravity bottle was used for this purpose, and it was washed, in the first place with sulphuric acid, secondly with distilled water, and thirdly with alcohol; when this was done it was found to contain mercury without any air-specks or any diminution of that metallic lustre which pure mercury exhibits when in contact with a vessel of clean glass. Three different specimens of pure mercury were used, and were separately weighed in the specific-gravity bottle at 62° Fahr. The following results were obtained :—

	Weighed in air.
Mercury from the cistern of the old Kew standard barometer, filling the bottle, weighed at 62° F.	grs. 13591·36
Mercury from the cistern of the new Kew standard barometer weighed at 62° F.	13591·66
Mercury used in experiments with air- thermometer weighed at 62° F.	13591·96

the mean of these will be 13591·66 grs.

It was found that the specific-gravity bottle had an internal volume equal very nearly to 4 cubic inches; and assuming that a cubic inch of air weighs 0·31 gr., then the air displaced by the liquid filling the bottle would weigh 1·24 gr.

In like manner the air displaced by the Kew standard weights (sp. gr. 8·2) would have the volume of 6·6 cubic inches, and would weigh 2·04 grs.

From these premises we find that the real weight of the mercury *in vacuo* would have been 13590·86 grs.

Again, the amount of water which the same bottle held at 62° F. weighed in air 1000·53 grs.

Here the air displaced by the bottle is, as before, 1·24 gr., while that displaced by the weights is only 0·15 gr.

From this we find that the real weight of water filling the bottle at 62° F. would be *in vacuo* 1001·62 grs. We have thus—

True weight of mercury filling the bottle at 62° F. = 13590·86 grs.

True weight of the same volume of water at 62° F. = 1001·62

And hence the specific gravity of mercury at 62° F., as compared with water at the same temperature, will be 13·569 nearly.

Again, if we assume the correctness of Regnault's Table of the absolute dilatation of mercury, and also that of Despretz's Table of the absolute dilatation of water, we shall find that the weight at 32° F. of a volume of mercury weighing 13590·86 grs. at 62° F. will be

$$13590·86 \times 1·00298 = 13631·361 \text{ grs.}$$

Also the volume at 4° C., or 39°·2 F., of a volume of water weighing at 62° F. 1001·62 grs., will be

$$1001·62 \times 1·0011437 = 1002·766 \text{ grs.}$$

Hence the specific gravity of mercury, according to the French method of determining it, will be

$$\frac{13631·361}{1002·766} = 13·594.$$

A determination by Regnault gives 13·596.

These two results agree very nearly with one another; and this agreement tends not only to verify the correctness of Regnault's determination, but to show that Regnault's Table of the dilatation of mercury, and Despretz's Table of the dilatation of water, agree together—a remark that had been previously made by Dr. Matthiessen in a paper which he recently presented to the Society.

GEOLOGICAL SOCIETY.

[Continued from p. 238.]

February 7, 1866.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read :—

1. "On the mode of formation of certain Lake-basins in New Zealand." By W. T. Locke Travers, Esq.

The author's observations had been chiefly directed to the neighbourhood of the Spencer Mountains, which occupy the centre of the area constituting the provinces of Nelson and Marlborough, in the Middle Island, and in this paper he more particularly described Lake Arthur, Lake Howick, and Lake Tennyson, with the rivers flowing out of them. After describing the nature and mode of occurrence of certain Postpliocene boulder-beds overlying older Tertiary deposits in the vicinity of Lake Arthur, Mr. Travers showed that that lake owes its existence to the presence of a moraine nearly a mile and a half in width, and extending for several miles down the valley. Similar facts were then described as having been observed at Lakes Howick and Tennyson; and attention was specially drawn to their great depth, Lake Howick being 1000 feet deep rather less than halfway up, and the others also attaining a depth of several hundred feet. The valleys of the rivers Dillon and Clarence present abundant evidence of the former existence of enormous glaciers in them; and these the author described in detail.

In conclusion Mr. Travers stated that, although he had confined his remarks to the lake-basins found amongst the spurs of the Spencer Mountains, he firmly believed that all the lakes which lie in the valleys of rivers debouching on the Canterbury plains owe their existence to moraine-dams which have the same foundations as the Postpliocene shingle of which the plains themselves are formed, and that therefore the sites of those lakes were occupied by ice at the commencement of the period of depression, and so continued for some time after the re-emergence of the upper part of the plains above the level of the sea.

2. "On the occurrence of dead Littoral Shells in the bed of the German Ocean, forty miles from the coast of Aberdeen." By Robert Dawson, Esq. Communicated by T. F. Jamieson, Esq., F.G.S.

The occurrence of shells of *Purpura lapillus*, *Litorina rudis*, *Solen siliqua*, and *Mytilus edulis*, in a worn and semifossil condition, at depths of 36, 40, and 42 fathoms, on the bank known as "the Long Forties," seemed to the author, in conjunction with other and well-known facts, to point to a time, towards the close of the Glacial period, when the British islands stood higher above the sea than they do at present. The fact of four species having been found in the course of one day's dredging was, Mr. Dawson considered, sufficient to render it probable that they had lived and died where they were found, and did not owe their presence at that depth and distance from land to any mere accident.

3. "On the Glacial Phenomena of Caithness." By T. F. Jamieson, Esq., F.G.S.

The glacial drift of Caithness occurs in sheets filling up the low

troughs and winding hollows which form the beds of the streams, the rocks on the higher ground being either bare or hidden by a growth of peat and heather. It thins out at altitudes of from 100 to 150 feet, and its thickness is therefore very variable, though it seldom much exceeds 100 feet. Mr. Jamieson first described the distribution of the drift-beds over the area in question, their texture and colour at the different localities where they occur, and the nature and appearance of the stones and boulders found in them; he then noticed the broken state of the shells, the most common species being *Cyprina Islandica*, *Astarte borealis*, *A. elliptica*, *Tellina calcarea*, *T. Balthica*, and *Turritella unguina*. The direction of the glacial markings on the rocks was shown to be pretty uniformly from N.W. to S.E. (true); so that it must have been produced by a movement of ice proceeding from an external region to the N.W., and not by glacier-action proceeding from the interior of the country, as is the case in the midland region of Scotland. The glacial drift of Caithness and the old boulder-clay of the middle of Scotland resemble one another in their physical arrangement, but differ in the prevalence of marine organisms in the former. The absence of tranquilly deposited glacial-marine beds, of moraines, and of gravel hillocks, and the deficiency of valley-gravel in Caithness, are also points in which the glacial series of that area differs from that of Central Scotland; and Mr. Jamieson inferred that, of the two series, the Caithness drift was the more recent. In conclusion the author described the deposits of the Postglacial period in Caithness, and showed that they did not differ materially from those occurring in the rest of Scotland.

XLVIII. *Intelligence and Miscellaneous Articles.*

ELECTRICAL CONDUCTIVITY OF GASES UNDER FEEBLE PRESSURES.

BY A. MORREN.

THE author alludes to the experiments which M. A. de la Rive made on the conductivity of nitrogen and hydrogen; and he publishes the results at which he himself has arrived either with these gases or with others.

The author enters into details on the subject of the methods which he used in his experiments. It is sufficient to mention that, in rarefying the gases, he used a mercury aspirator provided with two manometers, one a mercurial one, the other containing sulphuric acid, so that the pressure on the gas could easily be determined to one-tenth of a millimetre of mercury.

The electricity was furnished by a Ruhmkorff's coil put in action by four Bunsen's elements; and the intensity of the current was measured by a galvanometer formed of copper wire, 25 metres in length and one-tenth of a millimetre in diameter. The gas is slowly rarefied, so as to catch the moment of the first deflection; and at each instant the elastic force of the gas, and the corresponding deflection of the galvanometer, were accurately noted. To compare with greater certainty the results obtained with various gases, the author had to

construct and frequently verify the Table of the ratios of the forces to the galvanometric deflections.

M. Morren has succeeded thus in arranging a comparative Table of the electrical conductivity of some gases, which contains for each gas the pressure, the deflection of the galvanometer, and the force of the current corresponding to this deflection. Up to 36° of deflection the forces are represented by the same numbers as the deflections; but from 36° they increase far more rapidly than the deflections.

Thus, for a deflection of $66^{\circ} \cdot 2$ (the maximum deflection obtained with hydrogen under a pressure of 2 millims.), the corresponding force is 174, and so on.

One of the most singular characters which rarefied gases present is the black non-luminous band found near the negative electrode. Its length frequently varies under circumstances which apparently are quite identical; yet the author was able to determine the following lengths in various gases at pressures between 1 millim. and $0 \cdot 5$ millim:—

	millims.
Hydrogen	40
Nitrogen.....	35
Oxygen	64
Carbonic acid.....	19
Carbonic oxide	42

The colour of the jet varies with the nature of the glass of the tube in which the gas is enclosed, which exercises great influence on the colour which the different gases present. The aureole which surrounds the negative electrode varies least, and furnished a ready means of ascertaining the greater or less stability of some gases, particularly of the compound gases.

One fact noted by M. Morren, which appears curious enough, is that oxygen, when pure, and the vapour of mercury give no stratifications.

It follows from the numbers in the Tables given by M. Morren, that conductivity commences for each gas at very different moments as regards pressure. Thus the current begins to pass, approximately,

In hydrogen	at 60 millims. pressure.
In carbonic acid.....	„ 39 „
In air.....	„ 29 „
In nitrogen and in oxygen..	„ 23 „

It is seen from the same Table, that for each gas there is a certain pressure at which the electric conductivity is at its maximum. Thus this maximum is—

	millims.
For hydrogen 174, and takes place under a pressure of $2 \cdot 0^*$	
For oxygen 174	„ „ „ $0 \cdot 7$
For atmospheric air 172	„ „ „ $0 \cdot 7$
For carbonic acid.. 168	„ „ „ $0 \cdot 8$
For nitrogen. 162	„ „ „ $1 \cdot 0$

* From 2 to 1 millims. of pressure the intensity scarcely changes, since under a pressure of 1 millim. it is still 173.

Below the pressure at which the conductivity is at its maximum, this diminishes with the pressure. Thus under a pressure of 0.1 millim. the conductivity is still

150 for atmospheric air.

120 for hydrogen.

110 for carbonic acid.

110 for nitrogen.

86 for oxygen.

M. Morren attaches, with justice, great importance to the conductivities of nitrogen, oxygen, and atmospheric air; for it is probable that in the layer of greatest conductivity the phenomena of the electric light take place of which the atmosphere is the theatre. Now this layer, whose elastic force is 1 millim., ought, from Boyle and Mariotte's law, to be at a height of 8000 metres, and to have a thickness of 8000 metres also; that would be the zone of the phenomenon of the aurora borealis.

M. Morren concludes his memoir by some remarks, especially on the precautions to be taken to avoid the influence of moisture, and on the necessity, when using glass tubes, of working in dry weather, or at all events under the same hygrometric state.

He also gives some details of the attempts made to determine the conductivity of compound gases which the current decomposes. Their conductivity is generally very small, and commences late. Thus carbonic oxide commences to allow the current to pass only under a pressure of 11 millims., carburetted hydrogen under a pressure of 16 millims., and sulphurous acid under that of 5 millims. For this latter gas the greatest deflection is 54° ; it takes place under a pressure of 2 millims., and then descends rapidly. Cyanogen allows the current to pass under a pressure of 5 millims.; at 4 millims. the deflection is 7° , at 3 millims. 21° ; it then rapidly ascends to 38° , to 45° , and even to 49° . A powerful reaction then takes place, accompanied by an appreciable modification in the tint; from the time when the tint is modified, the stratifications appear with extreme delicacy, passing from the negative to the positive electrode with a magnificent undulating motion. The luminous phenomena which the passage of electricity in rarefied cyanogen present are very brilliant, and deserve to be studied more closely.—*Annales de Chimie et de Physique*, vol. iv. p. 325; *Bibliothèque Universelle*, January 20, 1866.

ST. ELMO'S FIRE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I have just received from Captain Briggs, the intelligent commander of the steamer 'Talbot,' the following account of the occurrence of this rare electrical phenomenon in the Irish Channel on the morning of the 7th inst. There has been of late a good deal of electrical disturbance in the atmosphere, and I have ascertained that

a thunderstorm passed over Cheshire on the evening of the 6th inst. The brush discharge, seen by Captain Briggs to issue from various parts of the ship, indicates a negative charge, either in the surrounding atmosphere or in the snow-flakes which were falling thickly at the time.

I am, Gentlemen,

Yours truly,

Royal Institution,
March 17, 1866.

E. FRANKLAND.

“On the morning of the 7th inst. we fell in with a heavy snow-storm off the Isle of Man, which lasted for three hours. During this time the steamer which I command exhibited the following phenomenon—a blue light at each mast-head, and also one from each gaff-end. Presently one was seen on the stem-head, and, being easily accessible, I had the opportunity of closely examining it. I found that the light, which appeared large at a distance, was made up of a number of jets, each of which expanded to the size of half-a-crown, appeared of a beautiful deep violet colour, and made a slight hissing noise. Placing my hand in contact with one of the jets, a sensible warmth was felt, and three jets attached themselves to as many fingers, but I could observe no smell whatever. The jets were not permanent, but sometimes went out, returning again when the snow was heaviest. This was from 1 to 3 A.M. At daylight I carefully examined the place, but no discoloration of the paint was to be seen. The stem in this part is wood, with iron plates bolted on each side, and it appeared to me that the jets came out between the wood and iron. The barometer stood at 29.10 in. The ship is an iron one, but I did not observe any alteration or other effect upon the compasses. I have seen the same phenomenon abroad, but never before in these latitudes.”

HISTORICAL NOTICE IN REFERENCE TO THE RETARDATION OF
THE EARTH'S VELOCITY OF ROTATION. FROM A LETTER AD-
DRESSED BY PROFESSOR FICK IN ZURICH TO PROFESSOR POG-
GENDORFF.

Some time ago, during a discussion on several points of the mechanical theory of heat, the question was mooted as to who was the first to assert that the velocity of the rotation of the earth must be retarded by the action of the sun and moon in producing tides. I have found that the priority in this respect does not belong to any living philosopher*. Kant developed it clearly and completely in a short essay which appeared in 1754, under the title “Investigation of the question which has been set for a prize for the current year by the Royal Academy of Sciences at Berlin, Has the earth undergone an elongation in the length of its axis?” This paper is in volume viii. of Hartenstein's edition of Kant's works, published at Leipzig in 1838, by Modes and Baumann.

* It is commonly assumed that Mayer first expressed this idea in 1848, in his essay ‘On Celestial Dynamics.’ Compare *Phil. Mag.* S. 4. vol. xxv.

ON SEA-LEVELS.

To the Editors of the Philosophical Magazine and Journal.

Kitlands, Dorking,
March 24, 1866.

GENTLEMEN,

I hope you can afford me space in your forthcoming Number to announce and correct a serious blunder in my paper on "Sea-levels."

Starting, as I did, I should have treated the excess of matter in the nucleus, equally with the ice-cap, as extraneous to the homogeneous spheroid of which I was determining the *form* only.

The term in δ would then have disappeared spontaneously from my equation; and its value, or the depth southward to which the nucleus will be sunk by the downward pressure of the ice-cap, would have to be determined by considering that, for equilibrium, there must be no tendency to relative motion between the rigidly-connected disturbing bodies and the homogeneous spheroid. And this would in fact be just to reinstate into the expression for

$$\int (Xdx + Ydy + Zdz)$$

the two terms in Q_1 which had been previously expunged, equating their sum to zero*.

The result is, that the rise of water (above the mean level of the then existing sea), measured from the centre of the nucleus, would be that calculated in my paper $+\frac{3}{\rho-1}\frac{\beta}{2}A_1Q_1$, or, with my numerical data, 375 cos N.P.D. in feet. Thus we obtain at the north pole a rise of 1078 feet instead of 703; at the south pole, a fall of 206 feet instead of a rise of 169; at latitude 55° N., a rise of 387 feet instead of 80; at latitude 55° S., a fall of 194 feet instead of a rise of 113.

D. D. HEATH.

THE AXIAL ROTATION OF THE EARTH.

BY J. S. STUART GLENNIE, M.A., F.R.A.S. ETC.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I observe that in the current Number of your Magazine Professor F. Guthrie proposes the hypothesis that "the earth revolves on its axis in the same direction as its orbital rotation," in consequence of its "side nearest the sun" encountering "æther of greater density than is met with by the remoter side."

I beg to say that in the Philosophical Magazine for April 1861, p. 277 *et seq.*, I had already stated, and applied to the case of the planetary (axial) rotations, the theorem that, "according as the resultant of a resisting medium passes, or not, through the centre of gravity of a revolving body, is it an accelerating force of revolution, or a partially neutralized accelerating force of rotation."

This theorem seemed to me of importance because of the general assumption that a resisting medium should affect the (orbital) revo-

* It is the same ceremony that Laplace goes through in respect of the actions of the particles of the spheroid itself on one another and on the centre: Liv. 3. ch. 4. No. 23; and again in the Theory of the Tides.

lutions, and not, as I contended, the (axial) rotations, of bodies of such size as to make the resultant of the resisting pressures pass otherwise than infinitesimally near the centre of gravity of the whole body. Professor Challis in particular finds this non-resistance of the æther to the orbital motions of the planets a problem requiring a special solution (Phil. Mag., May 1859).

As to Professor Guthrie's hypothesis of "the inequality of æthereal resistance being the real and only cause of the earth's axial rotation," I may add that, in the communication above referred to, I ventured to remark that "In the actual case of the planets, their masses and velocities of rotation are such that the solar medium can be of course conceived, not as causing, but only as tending slightly to accelerate their rotations." Were it the cause of such rotations, there would be such acceleration as, in the case of the earth at least, I think I may say that we know there is not.

But, in the hope that this short note may be yet in time for insertion in your next issue, I shall add nothing further on the equally difficult and interesting problem of Compound Translation and Rotation.

I am, Gentlemen,

Your obedient Servant,

Stone Buildings, Lincoln's Inn,
March 21, 1866.

J. S. STUART GLENNIE.

RELATION BETWEEN THE VARIATION OF SUN-SPOTS AND THAT OF THE AMPLITUDE OF MAGNETIC OSCILLATION. BY FATHER SECCHI.

I have just completed the reduction of the magnetic observations made during the years 1859-65, and of the sun-spots during the same period. The results are interesting, as showing the reciprocal influence of the two periodic variations, of the spots and of the amplitudes of the daily magnetic oscillation in our climates. The following is a summary:—

Years.	Days of observation of the sun-spots.	Number of groups observed.	Daily variation of the magnetic declination.	Variation of the horizontal intensity.
1859.	164	257	div. 8·105	div. 9·53
1860.	122	251	8·025	9·59
1861.	124	269	7·011	9·42
1862.	49	102	6·572	9·03
1863.	126	105	5·579	9·31
1864.	100	97	6·121	9·18
1865.	181	86	5·547	9·00

(Each division of declination is equal to $1'341$, and of horizontal force is equal to $0'00019$.)

The year 1862 is poor in observations. It is seen from this list that a minimum of spots corresponds to a minimum of magnetic variation.—*Comptes Rendus*, January 29, 1866.

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[FOURTH SERIES.]

MAY 1866.

XLIX. *On the Question of the Unit of Electrical Resistance.*

By WERNER SIEMENS*.

IN the year 1860 I published† a method by means of which I had succeeded in constructing exact resistance-standards. I then proposed to accept, as unit of conducting-power, that of mercury at 0°C., and as unit of resistance that of a prism of mercury one metre long and one square millimetre section, or a million times the resistance of a cubic metre of mercury at 0°C.

The grounds upon which I supported my proposition were briefly as follows:—

The adoption of an arbitrary material unit of electrical resistance, or one more or less inseparable from some natural measure to be set up somewhere like the normal metre and multiplied by copying it, is not advisable, because we have not sufficient guarantee of the electrical permanency of the materials; and were we even sure of the permanency of such a standard measure, there could be no question that repeated copying and re-copying in different materials, coupled with their possibly different behaviours in regard to molecular changes, would soon end in the distribution of faulty measures, as was the case, in so marked a degree, in the copies of Jacobi's normal standard.

The resistance-unit to be adopted must therefore consist of a definition, or be an absolute measure which can be at any time and in any place reconstructed. As such, Weber's dynamic resistance-unit would be well qualified for scientific purposes, if it could be reproduced with an exactness equal to what we are entitled to look for in the comparison of two different resistances.

* Communicated by the Author.

† Poggendorff's *Annalen*, vol. cx. p. 1. [Phil. Mag. S. 4. vol. xxi. p. 25.]
Phil. Mag. S. 4. Vol. 31. No. 210. May 1866. Z

But this will never be the case ; therefore Weber's unit cannot be adopted for general resistance measurements—although it is obviously of the last importance to determine, as exactly as possible, the relation between this and the unit to be chosen.

In the establishment of a general unit of resistance, its practical advantages, rather than the scientific harmony of systems of measurement, should be taken most prominently into consideration. And viewed in this light—seeing that determinations of resistances, combined with dynamic values, occur only very rarely and in strictly scientific instances, while the far overwhelming number of cases, on the contrary, consist of simple comparisons of the resistances of bodies of different sizes, shapes, and materials—a resistance-measure founded upon a corporeal basis gains many reasons for being preferred to a dynamic system.

It is by such considerations that the system proposed by me is recommended. In the definition of the unit, I have given the metre as the measure of space, and mercury as that conductor which is, without doubt, the best suited to serve as unit of conducting-power. The reproduction of this unit is practicable with sufficient exactness—indeed, when the greatest possible care is observed in manipulating, probably with an almost unlimited exactness.

Of the soundness of these grounds I have not yet found any disproof based upon internal shortcomings.

On the other hand, Dr. Matthiessen (in 1861) proposed, instead of mercury, a certain alloy of gold and silver as the resistance-unit—a system which any one practically acquainted with the difficulties attendant on the reproduction of a homogeneous alloy, on the forming it, when obtained, into wires of perfectly equal length and section and degree of hardness, without stretching or bending, and, lastly, on the soldering of the ends to thick connexion-terminals without altering the resistance, will scarcely at first sight be prejudiced in favour of.

I need not go further, however, into the merits or demerits of the system, as Dr. Matthiessen, at a later date, abandoned it as unit in favour of the proposition made by the Committee appointed by the British Association in 1861 to report on the most expedient unit for general adoption, of which Committee he is an active member.

The Committee has presented to the Association, at its different Meetings, four Reports, of which I have received those of 1862-64. In these Reports the theory of Weber's dynamic system of measurement, embracing the unit of mechanical effect introduced by Professor Thomson, is explained with much clearness, the great scientific importance of a general introduction

of this systematic and coherent method of measurement is very convincingly put forward, the method proposed by Professor Thomson for determining the $\frac{\text{metre}}{\text{second}}$ unit is developed, and, lastly, the *modus operandi* of the experiments which have been made, and the results obtained, are given in detail.

The names alone of William Thomson and Clerk Maxwell are a sufficient guarantee of the high scientific value of these researches. Indeed they have very far surpassed in exactness the previous determinations of Thomson and Weber.

The Committee has, notwithstanding, arrived at the conviction that even Weber's system is not adapted to a resistance-unit. In the first Report the proposition is made to take, as the normal measure, a material resistance-standard which shall have a value as near to 10^{10} Weber's units or $10^7 \frac{\text{metre}}{\text{second}}$ units as present means enable its determination. This normal measure is to remain unchanged, and, under the name "B. A. unit," to become the future measure of electrical resistance. From time to time its value is to be redetermined in absolute units and coefficients of correction, published for the use of physicists in calculating dynamical values.

The members of the Subcommittee—consisting of Professor Maxwell, Professor Matthiessen, and Mr. Jenkin, who are deputed to construct the normal standards and their copies—believe that they have met the objection that the resistances of the standards might change spontaneously by constructing ten different normals of alloys of the noble metals and of mercury, and by making the copies for distribution of an alloy of silver and platinum. According to Dr. Matthiessen, these alloys are not subject to spontaneous changes, whilst other metals and alloys are found to show considerable variations in the course of two years.

I do not in the least undervalue the importance of Dr. Matthiessen's experimental researches on this subject; but at the same time I do not hold that his expressed opinion (that the alloys of silver with either gold or platinum are electrically permanent) is so absolutely proved to be a law of nature as to warrant the foundation of normal resistance-standards, whose purpose is to last for all time, upon the strength of it. I am rather surprised that Dr. Matthiessen should have observed in German silver marked changes within short periods of time, as I have always found this alloy remarkably constant. But this only proves that in the conducting-powers there exist still many unknown factors which can only be brought to our knowledge by lengthened research.

It is true Dr. Matthiessen brings forward an argument in favour of the electric permanency of an alloy of silver with gold—

in which I can scarcely suppose him, however, to be serious: it is that we have never found a gold chain become brittle.

It may, however, be conceded that changes in the resistances of normal standards and their copies may be so small as to be without practical importance in our present experiments. But the normal measures of the B. A. are destined to serve in future times, when probably an infinite number of higher claims to exactness in a measure will be set up than we can pretend to.

On this ground it is certainly significant that the Committee should have made ten normal measures instead of a single one, even supposing them to agree with each other to within 0.03 per cent., as asserted.

With regard to the agreement between the B. A. unit and $10^7 \frac{\text{metre}}{\text{second}}$ units, even allowing that it is within 0.1 per cent., as stated in the Report of 1864, still it is too small to enable the values to be given out as equivalents; and if a coefficient must be used, I do not see that it matters in the least whether it differs a little more or a little less from unity.

Besides, it is by no means satisfactorily proved that this professed accordance of the B. A. and the $10^7 \frac{\text{metre}}{\text{second}}$ units really exists. A glance at the Table of results given in the Report of 1864 (p. 350) will suffice to show, between the values of the negative and positive observations, combined into pairs, differences amounting to 9 per cent. These differences are partly accounted for by the torsion of the fibre, which entered into the results, adding or subtracting from the observed values, according as the helix of the apparatus was rotated right- or left-handed. But the mean values of these pairs of observations differ still as much as 1.4 per cent. By what train of reasoning the Subcommittee holds itself justified, in the face of such differences between even the means of their single observations, in concluding upon a probable error of only 0.1 per cent., I am totally unable to imagine. Whatever method is taken to calculate the mean values of the given numbers, if some of the extreme measurements or some of the mean values be left out, very much greater differences are indicated.

In my opinion, certainty lies only within the limits of those numbers which are not liable to be regarded with mistrust as being faulty and therefore entitled to be cast out.

It is, however, impossible, from the numbers given in the Table in question, to conclude upon a degree of exactness so great between the B. A. and $10^7 \frac{\text{metre}}{\text{second}}$ units, as is done by the Subcommittee; and when we recollect that the observations were made with the same apparatus, with the employment of the same correction-coefficients, the probability is considerably

augmented that the difference between the two units is very much greater than it appears. It is true that the determinations of the constants are, for the most part, given as being exact within 0.0001; but we must conclude that this was only attained in consequence of extraordinary manipulation and carefully selected methods of measurement. The methods used are, however, not entirely free from sources of possible error. It is well known, for example, that it is absolutely impossible to wind a coil of silk-covered wire to an approximately round or solid coil without stretching it. The amount of this stretching varies, with the thickness and the tension of the wire (in winding), between 1 and 6 per cent. It would therefore be next to impossible to conclude, with anything like certainty, within $\frac{1}{2}$ per cent., upon the real length of the coiled wire. The real length of the coiled wire, however, is given as 311.2356 metres. In addition to this, it is quite impossible to wind such a coil perfectly round and concentric with covered wire. The circumference, the mean radius, the thickness of the coil, &c., can therefore only be given approximately. But in the Reports the numbers representing these values are given to within thousandths of a millimetre, and are supposed to be trustworthy to within a ten-thousandth part of their several values. There are other minor points by which errors might creep into the observations, which would, like the above, be constant for all the series made with the same apparatus, and others which might affect individual observations. Whether the determinations of the magnetic moment of the suspended needle and the horizontal component of the earth's magnetism at the time of making the observation can be carried to the same degree of accuracy, we must leave undecided.

It is very far from my intention to assert that these measurements were not in reality made with all the exactness which is ascribed to them; they can, however, in that case only be the result of processes which have no general currency.

Until these experiments have been repeated in other places, with other apparatus, by other observers, and the result found to agree with that of the Subcommittee, I think I am justified in regarding the B. A. unit as representing $10^7 \frac{\text{metre}}{\text{second}}$ units only to within some few per cent.

For this reason I still hold to my objections against the adoption of the material standards of the Subcommittee as the basis of a general resistance measure. In doing so, I do not in the least degree ignore the immense value of the experimental researches which the different members of the Subcommittee have contributed towards the solution of the questions at issue, and of the determination of Weber's unit, which, under the auspices of the

British Association, has undoubtedly been made with the greatest possible exactness. I am, on the contrary, certain that science has materially profited by a most serviceable work.

But I think that the Committee, on convincing itself that Weber's absolute unit even was not adapted for a normal measure, would have done better not to have set up another arbitrary unit, but to have had the mercury unit, as defined by me, reproduced with all possible care, with the great talent and the perfect arrangements at their command, and to have distributed copies of it. This unit is already very generally employed, and is found to answer perfectly all practical wants. The correction-coefficient to reduce it to absolute measure could then have been determined as exactly as possible and published. By this procedure the Committee would have conformed with Kirchhoff's proposition, published in the appendix to the first Report, that both measures should be retained, and not exclusively Weber's system—an opinion in which Wilhelm Weber himself perfectly coincided, and with which the Committee, in its first Report, declared itself satisfied.

There is no doubt that with the rich means at their command, the Subcommittee could have determined the mercury unit with an accuracy quite equal to present requirements—that is to say, equal to the comparison of two different resistances—as is proved as well by my original experiments as by the more recent and careful measurements by Robert Sabine.

Future and more complete determinations of the unit would become necessary as the exactness of physical measurements progresses. This, however, could scarcely give rise to inconvenience, as the true value of the unit may be definitely settled,—the differences which have shown themselves between the three determinations made in my laboratory being so small as to enable them to be neglected entirely in practice in ordinary measurements of electrical resistances; while for exact measurements it would be necessary to repeat from time to time the adjustment of the coils, on account of their possible want of permanency.

Unfortunately the Committee has not thought proper to follow the way which Kirchhoff and I proposed to them. On the contrary, two of its members (Professor Matthiessen and Mr. Fleming Jenkin) have attacked my proposition not only in the reports of the Committee, but also in special papers*, in a way not hitherto customary, I think, in scientific critiques. The plan followed by these gentlemen in common does not consist in opposing the principle of the system by any reasonable grounds, but in attacking the trustworthiness of my labours.

* Phil. Mag. S. 4. vol. xxix. p. 361, and p. 477.

Dr. Matthiessen concentrates his argument in the two theses—

1st. *That no true mercury unit has been issued.*

2ndly. *That the units issued from time to time have not the same resistance.*

He endeavours to justify both these assertions by the fact that, in the determinations of the unit, I made use of the wrong specific gravity of mercury; that two sets of resistance-coils shown in the London Exhibition of 1862 differed 1·2 per cent. between themselves; that in the first determination of the mercury unit differences of 1·6 per cent. appeared between the different tubes; and that his determination of the unit differs 0·8 per cent. from mine.

In supporting his first assertion, Dr. Matthiessen loses sight of the fact that the measure proposed by me consists of a definition, and is therefore absolute. That the resistance-coils and standards which I have issued from time to time correspond exactly with this definition I have never asserted; on the contrary, I have repeatedly expressed the wish that, sooner or later, experienced physicists would take the trouble to reproduce the measure according to the definition with the best means at command. Dr. Matthiessen would have been justified in his first assertion if my definition had been doubtful, or if the method laid down had been untrustworthy; but he has proved neither. I concede, however, willingly that the specific gravity of mercury which I admitted into the calculation is not correct. When (in 1858) I made experiments to ascertain if the mercury unit was capable of being reproduced with sufficient exactness, I found the number 13·557, and adopted it as correct, as the same value had been found by direct comparison of the heights of mercury and water in connected tubes. Unfortunately, in the reproduction of the unit made more recently with much greater care and with improved instruments, the same specific gravity was retained, and not that given by Regnault (13·596) and by Balfour Stewart (13·594), of the truth of which there can be no question.

According to this, the standards hitherto issued are 0·287 per cent. too great; and if we take the percentage increment of the resistance of the German silver which I have employed between 0°C and 100°C . = 2·72, these standards will not represent the unit at the temperature stated, but at $10^{\circ}\cdot 5$ higher.

I am desirous of allowing due credit to Dr. Matthiessen for having given occasion to this correction, which has notwithstanding no influence whatever upon the value of the system.

Dr. Matthiessen asserts in addition that the issues of coils from time to time do not represent the same unit.

The unit has been determined in my laboratory at three different times, each time with a nearer approximation to the true

value. Mr. Sabine, in a note to his paper on the unit, gave the following comparisons between the calculated values of tubes which had been used in previous determinations :—

No. of tube.	Original determination, 1859.	First reproduction, 1860.	Second reproduction, 1863.
3	555·87	555·99	556·05
5	193·56	193·73	193·73
7	1917·32	1917·54
8	2600·57	2601·46

The greatest difference therefore between the assigned values of these tubes by the first and last determination does not exceed 0·1 per cent.

From the first determination only a few standards and sets of resistance-coils were made, originally for our own use. Those sets of coils which have been made subsequently, for our own use or for sale, have been, without exception, adjusted according to the second determination of the unit. Lastly, from the mean values of the third determination I have had about a hundred standards adjusted, which have been presented to the various physicists, technologists, and scientific institutions, in the hope of furthering the adoption of a rational measure. When sent from my atelier these standards were exactly alike, and, if they have not since altered, agree to within 0·05 per cent. with the mercury unit when, allowing for the corrected specific gravity, at a temperature 10°·5 Cels. above that at which they were adjusted.

The proof advanced by Dr. Matthiessen in support of his assertion that the units which I have determined do not represent the same resistance, is based upon the measurement of two of my resistance-coils, of each from 1 to 10,000 units, by Mr. Fleeming Jenkin, who was employed by the jurors of the 1862 Exhibition to report upon the electrical instruments. Mr. Jenkin found a difference of 1·2 per cent. between the values of these two sets of resistances. Whether Mr. Jenkin measured correctly or not* is of very little importance; but it is perfectly incomprehensible how a man of Dr. Matthiessen's experience and knowledge of the subject could make use of a comparison between coils adjusted for practical use years ago, when the art of copying resistances was scarcely known, with resistance-standards carefully adjusted in 1864; and still more incomprehensible is it that he could have based such an assertion upon the uncorroborated evidence of the Jury-report of an Exhibition.

* The difference given by Mr. Jenkin as 1·2 per cent. really amounted to 1·8 per cent.

Dr. Matthiessen must be well aware of the fact that points of contact between solid metals have always a changing resistance; that therefore the contact-plugs of a set of resistance-coils through which, either wholly or in part, the current has to pass must give rise to a want of exactness. He will himself have frequently found and be fully able to appreciate the great difficulties in the way of summing up 5000 units for the first time in this way without considerable error creeping in.

The set of resistance-coils which Dr. Matthiessen calls "Siemens, London," was one of the first made for private use in 1859, by a very incomplete method of combining the several normal resistance-coils arranged by the weight system. The set of coils in question formed one of the branches of a Wheatstone's balance with which determinations were made during and after the laying of the Red-Sea and Indian cables, and was, on account of the historical interest attached to it as the first complete and practical balance of the kind, considered to be deserving of a place in the Exhibition; for by its help the hitherto crude way of testing submarine cables had been first reduced to exact method. This set of coils was subsequently readjusted by an improved system.

Dr. Matthiessen asserts, further, that the sets of coils adjusted by this improved system differ 0.5 per cent. from the standards of 1864. He concludes this from the measurements of a copper wire which Mr. Fleeming Jenkin, during the Exhibition of 1862, compared with the sets of coils. What temperature the copper wire had in the two measurements, made with an interval of some years between them, is not given. A difference of $1\frac{1}{2}$ degree Cels. would, however, completely account for the apparent discrepancy!

Under any circumstances, Professor Matthiessen and Mr. Fleeming Jenkin were not justified in using a single doubtful determination, made only by one of them and not avowedly for any scientific purpose, to force into the Tables in the Reports of the Committee, as well as in their own papers, the columns "Siemens, Berlin" and "Siemens, London," next to a column headed "Siemens, 1864" (containing the honest value of the unit), with the too evident purpose of throwing a false colouring upon the value of the work I had attempted, by making it appear that the coils issued by me from time to time have represented different resistances.

Analogous to this is the oft repeated assertion that between my determinations of the unit differences of 1.6 per cent. occur, and that this, therefore, must be the limit of its exactness.

It is true that in my first published experiments on the subject one such difference occurs. I gave at that time the cause

to which this might be attributed, viz. variations of temperature of the copper wire with which the tubes were compared, to 3°C. , and of the tubes themselves to 2°C. Besides this, Sabine has pointed out in his paper that these differences were ascribable principally to errors in the measuring-apparatus, being quantitatively plus or minus according as the balance-wire was bisected on one side or the other of the middle point. And even were the differences apparent in the first determination not to be satisfactorily explained, they would nevertheless say nothing against the system, because the tubes chosen were not so cylindrical as they might have been—the object of this determination not being so much to establish normal standards, as to prove that the proposed method was capable of doing so. For practical purposes, at that date, an exactness of 0.5 per cent. was considered amply sufficient; and indeed Dr. Matthiessen was himself contented with determinations of the conducting-powers of the metals which differed many per cent.

The little standards which I have distributed were all adjusted to within 0.05 per cent. of the mean value of the third reproduction, from the description of which it will be evident that, with normal weights and measures, the mercury unit is reproducible within the same limit of exactness.

I do not think that Dr. Matthiessen should have compared the results of his determination with mine, as he has departed, in several material points, from the method which I laid down.

The correction-formula for conicalness employed by Dr. Matthiessen regards the tubes as series of different cylinders, and not as cones, as in my formula. The coefficient is, however, very small, and would not account for the difference between the determinations, although it would be in the same direction. A more important deviation from the *modus operandi* which I followed, is in filling the tubes by dipping them into a trough of mercury, and lifting them out again by pressing the ends between two fingers. The soft skin of the points of the fingers would naturally be pressed into the openings, whereby the weighed contents of the tubes would be too small, and the calculated resistance therefore too large.

It is also barely possible that Dr. Matthiessen has omitted to invert the tubes in the bridge, and to take means of the readings in each direction.

At any rate, the difference 0.8 per cent. exists between his determination of the mercury unit and mine; and if it is not explained by these points of difference in our manipulation, they serve to show, at least, how little care Dr. Matthiessen has taken to follow in the footsteps of the method I had pointed out. On this account I do not think that the result of his de-

terminations can be put forward as a proof of the incorrectness of mine.

Mr. Fleeming Jenkin, in his paper* "On the New Unit of Electrical Resistance proposed and issued by the Committee," &c., brings forward no new features whatever, but turns the conclusions and experiments of Dr. Matthiessen to account in a still more extended way. The communication which he makes, that four of my 1864 standards were compared with four different copies of the B. A. unit by four different observers, with four different measuring-apparatus, giving the values 1·0456, 1·0455, 1·0456, and 1·0457 respectively, is interesting.

The mean value of these observations (1·0456), multiplied into the coefficient of correction for the right specific gravity of mercury, or

$$\frac{13\cdot596}{13\cdot557} \times 1\cdot0456 = 1\cdot0486,$$

is therefore the value of the B. A. unit in mercury units, or

$$1 \text{ mercury unit} = 0\cdot9536 \text{ B. A. units.}$$

In the present uncertainty of the true relation between the values of the B. A. unit and $10^7 \frac{\text{metre}}{\text{second}}$ units, we can translate the value of a resistance given in mercury units into terms of the 10^{10} Weber's unit, or $10^7 \frac{\text{metre}}{\text{second}}$ units measure by subtracting 5 per cent.

The historical sketch of the order in which different propositions for resistance-measures have been made, and the progresses in the field of resistance-measurements, with which Mr. Fleeming Jenkin commences his paper, induce me to make a few observations in order to correct some errors and omissions of things which interest me personally.

Complete sets of resistance-coils, from 1 to 100 units, each unit equal to the resistance of one geographical mile of copper wire of 1 line diameter, at a temperature of 20° C., have been, since 1848, made in the Berlin branch of my establishment, repeatedly described, and distributed far and wide. Mr. Jenkin says that "until about the year 1850 measurements of resistance were confined, with few exceptions, to the laboratory; but about that time underground telegraphic wires were introduced, and were shortly followed by submarine cables, in the examination and manufacture of which the practical engineer soon found the benefit of a knowledge of electrical laws."

Mr. Jenkin ought surely to be aware that, as early as 1847 and 1848, subterranean lines of considerable lengths were

* Phil. Mag. June 1865, p. 477.

laid down in Germany. In the construction of, and in the necessary determinations of faults in, these lines, by the methods I had given, "the practical engineer" had repeated opportunities of making exact resistance-measurements, and of fully learning to appreciate the use of a knowledge of the electrical laws which governed his work, even then.

Complete sets of resistance-coils, from 1 to 10,000 units, were arranged by the help of the weight system in 1859, and used extensively in the cable-tests which my brother (C. W. Siemens) and I made in England. Mr. Fleeming Jenkin cannot have forgotten that he himself made tests of the Indian cable, at Birkenhead, under my direction, with such resistance-coils.

In his sketch, Mr. Fleeming Jenkin should also not have forgotten to bring forward the fact that, in our report upon the Red-Sea line, in the year 1859, the relative resistances of the conductor and insulator of the cable were given in mercury units, and that the method we followed *to measure the resistance which the insulating covering offered to the electric current, and to compare it with that calculated from the specific resistance of the insulating material*, forms the foundation of the rational system of cable-testing introduced by us, and which, with very trifling modifications, is in general use.

Further, Mr. Fleeming Jenkin should not have passed over in silence the paper read by my brother before the Eighteenth Meeting of the British Association, in which our systems of cable-testing, both before, during, and after laying the cable, are fully developed. I have yet to learn that any practical methods of determining faults in cables, besides those proposed by me, exist.

Speaking of the mercury unit, Mr. Fleeming Jenkin gives Marié-Davy (without, however, quoting any published matter) the honour of being the first who proposed mercury as a material adapted to the construction of a normal resistance-measure, and affords me only the credit of having materially furthered the exactness of measurements by the great care paid to the adjustment of my coils and apparatus. He is, however, silent as to the fact that they who had previously spoken of mercury as a fitting material gave no clue whatever to a method by which exact standards might be constructed with it.

In conclusion, Mr. Fleeming Jenkin must himself allow that his historical sketch is remarkably incomplete.

L. *On the Aqueous Lines of the Solar Spectrum.*

By JOSIAH P. COOKE, Jun.*

A CAREFUL examination of the solar spectrum, continued during several months with the spectroscope described in a recent article in this Journal† has led me to the conclusion that a very large number of the more faint dark lines of the solar spectrum, hitherto known simply as *air*-lines, are due solely to the *aqueous vapour* of our air, and hence that the absorption of the luminous solar rays by the atmosphere is chiefly at least owing to the aqueous vapour which it contains.

The appearance of the Fraunhofer's line D, seen under precisely the same conditions, but with increasing quantities of aqueous vapour in the atmosphere, is shown in figs. 1, 2, 3, and 4. The D line is selected because, being a favourite test-object for the spectroscope, its general appearance is well known to all observers. But even more marked changes than those here illustrated have been noticed in others, but chiefly in contiguous portions of the solar spectrum.

These changes attracted my attention from my earliest observations with the spectroscope; but with my first instrument and the bisulphide-of-carbon prisms then employed, it was almost impossible to eliminate the effects which might be caused by the variations in the condition of the instrument itself; and as these were known to be very great, it was possible that they might account for all the variations observed. With the improved instrument, however, just referred to, absolute constancy of action is obtained, and all merely instrumental variations avoided.

A peculiar condition of the atmosphere gave the first clue as to the cause of the changes under consideration. The weather on the 17th of November, 1865, at Cambridge, Massachusetts, was very unusual, even for that peculiar season known in New England as the Indian Summer. At noon the temperature on the east side of my laboratory was 70° F., while the wet-bulb thermometer indicated 66°, showing an amount of moisture in the atmosphere equal to 6.57 grains per cubic foot. At the same time the atmosphere was beautifully clear, and the sun shone with its full splendour. I have never seen the aqueous lines of the spectrum more strongly defined than they were on this day, and the total number of lines visible in the yellow portion of the spectrum was at least ten times as great as are ordinarily seen. The appearance of the D line on that day is shown in fig. 4. Between the two familiar broad lines D₁ and D₂ there were eight sharply-defined lines of unequal intensity, which is only

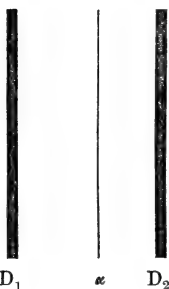
* From Silliman's Journal for March 1866.

† February 1866, p. 110.

very imperfectly represented by the woodcut. In addition to these, on the more refrangible side of the space between the two

Fig. 1.

January 5, 1866.
Temperature 10° F.
Dew-point $1^{\circ}5$ F.



Weight of vapour in one }
cubic foot of air } 0.81 grs.

Fig. 2.

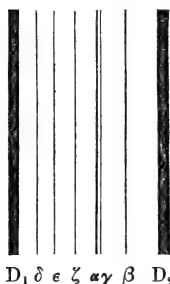
December 25, 1865.
Temperature 46° F.
Dew-point $33^{\circ}4$ F.



Weight of vapour in one }
cubic foot of air } 2.42 grs.

Fig. 3.

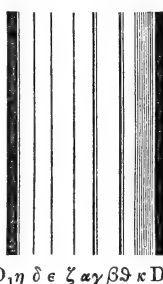
December 26, 1865.
Temperature 55° F.
Dew-point 46° F.



Weight of vapour in one }
cubic foot of air } 3.76 grs.

Fig. 4.

November 17, 1865.
Temperature 70° F.
Dew-point 64° F.



Weight of vapour in one }
cubic foot of air } 6.57 grs.

D lines, there was a faint but broad nebulous band, barely resolvable into lines of still lower magnitude*. It is impossible to represent this band accurately with a woodcut; and the shaded broad band marked κ , on the right-hand side of fig. 4, only serves to indicate its position and approximate breadth.

The 26th of December was also a warm day for the season, with a brilliant sun. At 1 p.m. the dry-bulb thermometer marked 55° , the wet-bulb 50° , and hence the amount of mois-

* We use this word in the same sense in which it is used by astronomers in reference to the fixed stars.

ture in the atmosphere was 3.76 grains per cubic foot. The appearance of the D line at this time is shown in fig. 3. Two of the lines, η and ξ , and the nebulous band κ , seen on the 17th of November, were invisible, and, moreover, the group of three lines, δ , ϵ , ζ , on the left-hand side of the figure, were only just within the limits of visibility.

On the 25th of December only two lines were visible within the D lines, marked α and β in fig. 2, and the last of these was quite faint. The temperature at the time of observation was 46° ; the wet-bulb thermometer indicated 40° , and the amount of moisture in the air was 2.42 grains per cubic foot. The sky was clear, and the sun brilliant. Lastly, on January 5, 1866, one of the clear cold days which are so common in our climate during the winter, only the single line α was visible within the D line, as is shown in fig. 1. At the time of observation (near noon) the dry-bulb thermometer marked 10° , the wet-bulb 9° ; and hence the amount of moisture in the atmosphere was only 0.81 of a grain per cubic foot. The sun, however, was as brilliant as in either of the previous cases. The D line also appeared as in fig. 1 on the 8th of January, 1866, when the thermometer at noon stood at 10° below zero Fahrenheit, and when the barometer attained the unusual height of 31 inches.

The above figures have been drawn so as to show, as nearly as possible, the relative intensity of the different lines under different atmospheric conditions. As no accurate means of making the comparison are yet known, I was obliged to depend upon my eye alone; and small differences at different times of observation may easily have escaped my notice. Indeed I should have been liable to great error were it not for the fact that one of the lines within the D lines, marked α in all the figures, does not vary in intensity, and served as a constant standard in making the observations. This is the only line which is given by Kirchhoff in his chart of the solar spectrum between the two D lines, and it is referred by him to the nickel-vapour, as the D lines themselves are to the sodium-vapour, in the sun's atmosphere. It is an undoubted solar line, and has been drawn with the same strength in all the figures in order to show that it is invariable.

With a very dry atmosphere the line α is the only one which appears within the D lines, as shown in fig. 1. As the amount of vapour increases, the line β makes its appearance. At first it is barely visible; but as the amount of vapour increases still further it becomes more and more prominent, until at last, as shown in fig. 4, it is even more intense than the line α . A careful comparison of these two lines might indeed serve as an approximate measure of the amount of vapour in the atmosphere;

and a series of comparisons made under the same conditions, at different heights, would give data for determining the law according to which the amount of vapour decreases with the elevation above the sea-level.

All the aqueous lines change in intensity like the line β . They first appear very faintly when the amount of vapour in the air reaches a definite point, varying for the different lines, and gradually gain in intensity as the amount of vapour increases. Thus the group of three lines, δ , ϵ , ζ , do not appear in fig. 2, are barely visible in fig. 3, but become very marked in fig. 4*. The lines η and ϑ and the nebulous band κ do not appear until the air is very moist, and even when it contains 6.57 grains of vapour per cubic foot they are still very faint. Under still more unusual atmospheric conditions they will undoubtedly become more intense, and we shall then probably be able to completely resolve the nebulous band and count the lines of which it consists.

It is hardly necessary to repeat that the examples here given are selected from a large number of observations. During the cold dry weather of winter the appearance of the D line is uniformly as shown in fig. 1, the line β only occasionally appearing when the atmosphere becomes more moist. During the warm weather of summer, when the absolute amount of moisture in the air is in almost all cases greater than in winter, the appearance of the D line is as uniformly that shown in fig. 3. It is only very rarely in the dry climate of New England, even during the summer, that all the lines shown in fig. 4 are visible, and, as already stated, I never saw them before so sharply defined as on the 17th of November last. Several conditions must evidently concur in order that the aqueous lines may be developed in their greatest intensity. In the first place, the air must be charged with vapour not only near the surface of the earth, but also through a great height of the atmosphere. Local causes might greatly increase the amount of moisture in the lower strata of the atmosphere and affect powerfully the hygrometer, which would not, to the same extent at least, influence the indications of the spectroscope. In the second place, other things being equal, the intensity of the aqueous lines must be strengthened by increasing the length of the path of the sun's rays through the atmosphere; and this is the longer the lower the altitude of the sun. But then, again, the intensity of the light has such an important influence on the definition of the lines, and the slightest haze in the atmosphere so greatly

* With an increasing quantity of vapour in the atmosphere the line γ of fig. 3 is seen before the group of lines δ , ϵ , ζ ; and an intermediate figure between figs. 2 and 3 might be given, showing only the lines D_1 , α , γ , β , D_2 .

impairs the distinctness, that I have generally found that the aqueous lines are seen best when the sun is near the meridian. Hence with an equal amount of moisture in the atmosphere, the late autumn may be a more favourable season for seeing the aqueous lines than the summer; for not only must the solar rays, when most brilliant at noon, traverse a greater extent of air, but, moreover, the atmosphere at this time is usually clearer, and the reflected beam of light which enters the spectroscope is at times even more brilliant than when the sun attains a higher elevation, and the light is reflected under less favourable conditions.

In the examples cited above, the comparisons were made under as nearly as possible the same conditions, so as to eliminate all causes of variation except the one under consideration. Days were selected when the atmosphere was perfectly clear and the sun's light, so far as I could judge, equally brilliant. Moreover the position of the spectroscope and mirror remained unchanged during the whole time. This mirror, which is used for reflecting the sun's light upon the slit of the spectroscope, is so arranged that it can be turned into any position by the observer while his eye is at the eyepiece of the spectroscope; and it was always carefully adjusted to the position of best definition at each observation. The manipulation of the mirror is fully as important in the use of the spectroscope as it is in microscopy.

It will be of course understood that the power of developing these faint aqueous lines depends very greatly on the optical capabilities of the spectroscope, and that the figures here given are relative to the instrument used in the observations. This instrument has been fully described in the article already cited. It is sufficient for the present purpose to state that it is provided with nine flint-glass prisms of 45° refracting angle, which bend the rays of light corresponding to the D line through an angle of $267^\circ 37' 50''$, and that corresponding to the H₁ line through an angle of $260^\circ 42' 20''$, when each passes through the prisms at the angle of minimum deviation. The dispersive power, therefore, of the instrument for these two rays is equal to $13^\circ 4' 30''$, and the rays corresponding to the two D lines are separated $1' 10''$. The object-glasses of the two telescopes of this spectroscope are $2\frac{1}{4}$ inches in diameter, and have a focal length of $15\frac{1}{2}$ inches; and, lastly, the size of the prisms and of the various parts of the instrument is adapted to these dimensions. With a more powerful instrument, a larger number of aqueous lines would be seen under the same atmospheric conditions. My own instrument has a set of sulphide-of-carbon prisms which disperse the light nearly twice as much as the

flint-glass prisms. These sulphide-of-carbon prisms are very variable in their action; but under the best conditions they might show the D line as in fig. 3, when with the flint-glass prisms it would appear as in fig. 2.

The facts stated in this paper fully account for the discrepancies in the representations which different observers have given of the D line. Some time since Mr. Gassiot of London gave in the 'Chemical News' a representation of the D line as seen with his instrument, showing several lines in addition to those seen by myself and other observers. On visiting the Kew Observatory in the summer of 1864, I was surprised to find that this instrument was less powerful than the one I was then using; and I also learned that these lines were only seen on a single occasion. The moist climate of England is the evident explanation of the additional lines.

As I stated at the beginning of this paper, the D line has been selected simply to illustrate a general truth. The development of aqueous lines in contiguous portions of the spectrum is even more marked than in the exceedingly limited portion here represented. Indeed, as has been already intimated, the number of these lines seen in the yellow region of the spectrum, on the 17th of November, was at least ten times as great as that of the true solar lines. That part of the yellow of the spectrum which lies on the more refrangible side of the D line, and in which, during dry weather, only a comparatively few lines can be distinguished, was then as thickly crowded with lines as the blue or the violet; but the lines were of course far less intense.

Professor Tyndall of London has shown, by a remarkable series of experiments with the thermo-multiplier, not only that aqueous vapour powerfully absorbs the obscure thermal rays, but also that the elementary gases of the atmosphere exert little or no action upon them. I have endeavoured to establish in this paper, from direct observations with the spectroscope, a similar truth in regard to the luminous rays. It has been estimated by Pouillet and others, that about one-third of the solar rays intercepted by the earth are absorbed in passing through the atmosphere; and it now appears that aqueous vapour is the most important, if not the chief, agent in producing this result. It is impossible, however, from any data we yet possess, to determine how great a power of absorption is exerted by the oxygen and nitrogen gases which constitute the great mass of our atmosphere. I have shown that a very great many, and I have no doubt that almost all the lines hitherto distinguished as air-lines are simply aqueous lines; but it is very difficult to distinguish atmospheric lines from the true solar lines, and our knowledge of the first is as yet very incomplete. It still remains to make

careful comparisons throughout the whole extent of the spectrum, before we can determine absolutely the relative absorbing power of the different constituents of our atmosphere.

One other inference from the facts here developed is worthy of notice before closing this paper. It has been for some time suspected that the blue colour of the sky was in some way connected with the vapour in the atmosphere; and it is a fact of common observation that this colour is more intense during the moist weather of summer than during the dryer weather of winter. The distribution of the aqueous lines through the solar spectrum not only confirms the opinion previously entertained, but also points to the cause of the colour. So far as my observations have extended, the aqueous lines are almost wholly, if not completely, confined to the more refrangible portions of the spectrum. Here they are found in vast numbers, and I am not positive that they exist anywhere else. If, then, the aqueous vapour absorbs most powerfully the yellow and red rays of the spectrum, the blue colour of the sky is the necessary result. The colour is therefore due to simple absorption, and not to repeated reflexions from the surface of drops of water, as some physicists have supposed.

As can readily be seen, the aqueous lines of the solar spectrum present a very wide field for investigation, but one which can only be cultivated under peculiar atmospheric conditions. This paper is only intended to open the subject. I hope to be able to continue the study on every favourable opportunity, and shall take pleasure in communicating, through the pages of this Journal, any future results.

Cambridge, Jan. 9, 1866.

LI. *On the Motion of a small Sphere acted upon by the Undulations of an Elastic Fluid.* By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

THE hydrodynamical problem which forms the subject of this communication, and which I have now had under consideration for a long series of years, is beset with peculiar difficulties, arising from the high order of the differential equations by which alone it can be solved, and from the imperfect state of our knowledge of the appropriate rules for drawing inferences from their integrals. The analytical treatment of questions relating to the motion and pressure of fluids leads in general to differential equations containing five variables; the problem I am about to discuss requires the solution of an equation containing four variables, and in no case can the number of variables be less than

* Communicated by the Author.

three; whereas in almost all the physical questions to which analysis has hitherto been applied, methods of solution have been employed which involve differential equations between the variables such that they are reducible to equations each containing no more than *two* variables. On account of the comparatively few applications that have been made of partial differential equations, I have derived little assistance in my hydrodynamical researches from antecedent or contemporary mathematicians, and have been compelled to attempt the discovery of new processes. When in doing so I have ascertained that an adopted course of reasoning has proved fallacious, I have not hesitated to indicate the reason of the failure, considering that every such indication tends to clear the way for finding the true course. Instances of such rectifications of previous methods will occur in the present communication, the special object of which is to point out the course of analytical reasoning which is alone compatible with the assumed properties of the fluid and the given conditions of the problem, and to exhibit as succinctly and connectedly as possible the particular processes required for its solution. What is here attempted is a mere matter of reasoning from admitted premises: in another communication I shall endeavour to show that the results to which the reasoning leads are absolutely necessary for the progress of theoretical physics.

In the course of the Supplementary Researches (Parts I., II., and III.) contained in the Philosophical Magazine for September and October 1865, and January 1866, certain conclusions are drawn which it will be convenient to cite here, as they bear essentially on the present investigation. First, the expression for the velocity (w) along an axis about which the condensation and transverse vibration are symmetrically disposed having been obtained, and the relation between that velocity and the condensation (σ), to terms of the second order, being found to be

$$a\sigma = \kappa w + (\kappa^2 - 1) \frac{w^2}{2a},$$

it was inferred, the motion being wholly vibratory, that the condensation corresponding to the second term of this equation has the effect of making the excursion of a *given* particle, when in the condensed half of a wave, equal to its excursion when in the rarefied half.

Secondly, reasons were given for concluding that, although the series for w and σ were derived from equations that are not linear, the velocities and condensations expressed by different sets of such series may *coexist*. As, however, it appeared from further consideration that the reasons alleged were not valid to the extent I supposed, I shall now enter upon a new investiga-

tion in order to ascertain how far the law of the coexistence of vibrations holds good when terms of the second order are included. For the present purpose it is not necessary to carry the inquiry further.

Let it be supposed that $udx + vdy + wdz$ is an exact differential ($d\psi$) for vibrations relative to an axis; and, regard being had to the series for the velocity along the axis previously obtained on that supposition, let us assume that for *any* distance from the axis we have

$$\psi = -\frac{mf}{q} \cos q\zeta - \frac{m^2 Ag}{2q} \sin 2q\zeta + \frac{m^3 Bh}{3q} \cos 3q\zeta,$$

q being put for $\frac{2\pi}{\lambda}$, ζ for $z - a't + c$, A for $\frac{2\kappa}{3a(\kappa^2 - 1)}$, and B for $\frac{3(7\kappa^2 + 1)}{32a^2(\kappa^2 - 1)^2}$, and f, g, h being functions of r and constants.

These values of A and B are extracted from a formula given near the beginning of Part I., where also will be found the following relation between a' , the rate of propagation, and a :—

$$\frac{a'^2}{a^2} = 1 + \frac{b^2}{q^2 a^2} + \frac{m^2}{12a^2} \cdot \frac{5\kappa^2 + 3}{\kappa^2 - 1}.$$

Since a' and m are constants, $\frac{b^2}{q^2 a^2}$ is a numerical constant, being in fact, as has been already shown, the quantity $\kappa^2 - 1$. Hence, if $\frac{b^2}{a^2} = 4e$, and $\frac{a'^2}{a^2} = \kappa'^2 = 1 + \frac{4e'}{q^2}$, we have

$$\kappa'^2 = \kappa^2 + \frac{m^2}{12a^2} \cdot \frac{5\kappa^2 + 3}{\kappa^2 - 1},$$

$$e' = e + \frac{m^2 e}{12a^2} \cdot \frac{5\kappa^2 + 3}{(\kappa^2 - 1)^2}.$$

On the supposition that $(d\psi) = udx + vdy + wdz$, we have, as is known, the exact differential equation

$$\left. \begin{aligned} 0 = a^2 \cdot & \left(\frac{d^2 \psi}{dx^2} + \frac{d^2 \psi}{dy^2} + \frac{d^2 \psi}{dz^2} \right) - \frac{d^2 \psi}{dt^2} \\ & - 2 \frac{d\psi}{dx} \frac{d^2 \psi}{dx dt} - 2 \frac{d\psi}{dy} \frac{d^2 \psi}{dy dt} - 2 \frac{d\psi}{dz} \frac{d^2 \psi}{dz dt} \\ & - \frac{d\psi^2}{dx^2} \frac{d^2 \psi}{dx^2} - \frac{d\psi^2}{dy^2} \frac{d^2 \psi}{dy^2} - \frac{d\psi^2}{dz^2} \frac{d^2 \psi}{dz^2} \\ & - 2 \frac{d\psi}{dx} \frac{d\psi}{dy} \frac{d^2 \psi}{dx dy} - 2 \frac{d\psi}{dx} \frac{d\psi}{dz} \frac{d^2 \psi}{dx dz} - 2 \frac{d\psi}{dy} \frac{d\psi}{dz} \frac{d^2 \psi}{dy dz}. \end{aligned} \right\} (A)$$

By substituting the foregoing value of ψ , I found, by calculations somewhat intricate, that this equation is satisfied to terms inclusive of m^3 , if f , g , and h have values expressed by the following series :—

$$\begin{aligned}
 f &= 1 - er^2 \\
 &+ \frac{e^2 r^4}{4} \cdot \left\{ 1 - \frac{m^2}{a^2} \left(\frac{5 - \kappa^2}{6(\kappa^2 - 1)^2} + \frac{9\kappa^2 - 29}{24} \right) \right\} \\
 &- \frac{e^3 r^6}{36} \cdot \left\{ 1 - \frac{m^2}{a^2} \left(\frac{17 - 11\kappa^2}{3(\kappa^2 - 1)^2} + \frac{147\kappa^2 - 247}{24} \right) \right\} + \&c. \\
 g &= 1 - er^2 - \frac{3\kappa^2 - 1}{4} e^2 r^4 + \frac{12\kappa^2 + 1}{18} e^3 r^6 + \&c., \\
 h &= 1 - er^2 - \frac{3}{32} \left(\frac{73\kappa^2 - 17}{4(\kappa^2 - 1)^2} - \kappa^2 + 5 \right) e^2 r^4 + \&c.
 \end{aligned}$$

(It should here be noticed that the value of f , or g , obtained in the January Number is incorrect.) These equations prove that for points near the axis it is legitimate to suppose, as I have done in previous investigations, that $\psi = f\phi$, ϕ being a function of z and t . As the foregoing reasoning may be carried on *ad libitum*, we may conclude that for this kind of motion $udx + vdy + wdz$ is a complete differential for the *exact* values of u , v , and w , and that the motion, being *characterized* by this analytical condition, is independent of arbitrary disturbances. Since it has been shown in the Number of the Philosophical Magazine for May 1865 that the constant κ is determined by the equation $\kappa^6 - \kappa^4 = 1$, it is easy to convert the foregoing factors that are functions of κ into numerical quantities.

By means of this determination of the function ψ , and the equation

$$a^2 \text{ Nap. log } \rho + \frac{d\psi}{dt} + \frac{1}{2} \left(\frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) = 0, \quad (\text{B})$$

the values of the velocities w and ω respectively parallel and perpendicular to the axis, and the condensation σ , may be found for a point at any distance from the axis. For our present purpose it will suffice to regard m as extremely small compared to a , and to restrict the calculation to terms involving m^2 . Accordingly the velocity and condensation at any position and at any time are given by the following equations :—

$$\begin{aligned}
 f &= 1 - er^2 + \frac{e^2 r^4}{4} - \frac{e^3 r^6}{36} + \&c., \\
 g &= 1 - er^2 - \frac{3\kappa^2 - 1}{4} e^2 r^4 + \frac{12\kappa^2 + 1}{18} e^3 r^6 + \&c.,
 \end{aligned}$$

$$\begin{aligned}
 w &= mf \sin q\zeta - \frac{2m^2\kappa g}{3a(\kappa^2-1)} \cos 2q\zeta, \\
 \omega &= -\frac{m}{q} \frac{df}{dr} \cos q\zeta - \frac{m^2\kappa}{3qa(\kappa^2-1)} \frac{dg}{dr} \sin 2q\zeta, \\
 \sigma &= \frac{m\kappa}{a} f \sin q\zeta - \frac{2m^2\kappa^2g}{3a^2(\kappa^2-1)} \cos 2q\zeta, \\
 &\quad + \frac{m^2(\kappa^2-1)}{2a^2} f^2 \sin^2 q\zeta - \frac{m^2}{2q^2a^2} \frac{df^2}{dr^2} \cos^2 q\zeta.
 \end{aligned}$$

As $\zeta = z - a't + c$, the velocity of propagation is a' , for determining which we have the equation $a' = \kappa'a$. The complete value of κ' involves, as we have seen, the constant m ; and it might hence be supposed that the rate of propagation is different for different sets of vibrations. But as there are no *a priori* conditions which determine the value of m to be different for different *simple* vibrations, it may be assumed to have the same value for all; and consequently different composite vibrations, the components of which have all the same value of q , although they may differ in magnitude according to the number and phases of the components, will be propagated with the same constant velocity $\kappa'a$. Also composite vibrations differing as to the value of q will be propagated with that velocity. The last two inferences are readily seen to be true, if only the first power of m be taken into account and the equations are consequently linear with constant coefficients. We have now to inquire how far they are true when terms of the second order are included.

For this purpose let us assume that

$$\psi = -m \cdot \Sigma \cdot \left[\frac{f}{q} \cos q\zeta \right] - m^2 A \cdot \Sigma \cdot \left[\frac{g}{2q} \sin 2q\zeta \right] + m^2 Q,$$

the first symbol Σ embracing an unlimited number of terms for which f , q , and c may be different, and the second an unlimited number of terms for which g , q , and c may be different. On substituting this value of ψ in the equation

$$\left. \begin{aligned}
 0 &= a^2 \cdot \left(\frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} + \frac{d^2\psi}{dz^2} \right) - \frac{d^2\psi}{dt^2} \\
 &\quad - 2 \frac{d\psi}{dx} \frac{d^2\psi}{dxdt} - 2 \frac{d\psi}{dy} \frac{d^2\psi}{dydt} - 2 \frac{d\psi}{dz} \frac{d^2\psi}{dzdt}, \quad \} \quad \cdot \quad (C)
 \end{aligned} \right\}$$

it will be found that that equation is satisfied if the values of f , g , and Q be determined by the following conditions:—

$$\frac{d^2 f_1}{dr^2} + \frac{df_1}{rdr} + 4e_1 f_1 = 0,$$

$$\frac{d^2 g_1}{dr^2} + \frac{dg_1}{rdr} + 16e_1 g_1 = -\frac{2\kappa}{Aa} \left(\frac{df_1^2}{dr^2} - f_1^2 q_1^2 \right),$$

$$\frac{a}{\kappa} \left(\frac{d^2 Q}{dx^2} + \frac{d^2 Q}{dy^2} + \frac{d^2 Q}{dz^2} - \frac{1}{a^2} \frac{d^2 Q}{dt^2} \right) =$$

$$\Sigma \cdot \left[\begin{aligned} & \sin(q_1 \zeta_1 + q_2 \zeta_2) (q_1 + q_2) \left(\frac{df_1}{q_1 dr} \frac{df_2}{q_2 dr} - f_1 f_2 \right) \\ & + \sin(q_1 \zeta_1 - q_2 \zeta_2) (q_1 - q_2) \left(\frac{df_1}{q_1 dr} \frac{df_2}{q_2 dr} + f_1 f_2 \right) \end{aligned} \right].$$

Here f_1 , e_1 , g_1 , and q_1 are the values of f , e , g , and q for any one value of λ ; and in the last equation q_1 , ζ_1 , f_1 , and q_2 , ζ_2 , f_2 are the values of q , ζ , and f for any two values of λ . The first two equations prove that the series for f_1 and g_1 are respectively of the same form as the series above for f and g . The equation for determining Q may evidently be satisfied by assuming that

$$Q = \Sigma \cdot [R \sin(q_1 \zeta_1 + q_2 \zeta_2) + S \sin(q_1 \zeta_1 - q_2 \zeta_2)],$$

R and S being functions of r , the values of which may be obtained by the method of indeterminate coefficients, every such function as f_1 and f_2 having been previously determined.

Having thus found for ψ an expression which is applicable to composite vibrations relative to an axis common to all the components, we may proceed to deduce expressions for the composite longitudinal and transverse vibrations (w' and ω'), and the composite condensation (σ'). By means of the equation (B) the following results will be obtained without difficulty:—

$$w' = m \Sigma \cdot [f \sin q \zeta] - \frac{2m^2 \kappa^5}{3a} \Sigma \cdot [g \cos 2q \zeta] + m^2 \frac{dQ}{dz},$$

$$\omega' = -m \Sigma \cdot \left[\frac{df}{qdr} \cos q \zeta \right] - \frac{m^2 \kappa^5}{3a} \Sigma \cdot \left[\frac{dg}{qdr} \sin 2q \zeta \right] + m^2 \frac{dQ}{dr},$$

$$\sigma' = \frac{\kappa w'}{a} + \frac{m^2}{2a^2 \kappa^4} (\Sigma \cdot [f \sin q \zeta])^2 - \frac{m^2}{2a^2} \left(\Sigma \cdot \left[\frac{df}{qdr} \cos q \zeta \right] \right)^2.$$

In these equations κ^4 has been put for $\frac{1}{\kappa^2 - 1}$, the equality of the two quantities having been proved by the equation $\kappa^6 - \kappa^4 = 1$. Now, since

$$(\Sigma \cdot [f \sin q \zeta])^2 = \Sigma \cdot [f^2 \sin^2 q \zeta] + 2 \Sigma \cdot [f_1 f_2 \sin q_1 \zeta_1 \sin q_2 \zeta_2], \text{ and}$$

$$\left(\Sigma \cdot \left[\frac{df}{qdr} \cos q \zeta \right] \right)^2 = \Sigma \cdot \left[\frac{df^2}{q^2 dr^2} \cos^2 q \zeta \right] + 2 \Sigma \cdot \left[\frac{df_1}{q_1 dr} \frac{df_2}{q_2 dr} \cos q_1 \zeta_1 \cos q_2 \zeta_2 \right],$$

it follows, if Q_1 be a function similar in form and composition to Q , and if regard be had to the foregoing values of w , ω , and σ , that

$$w' = \Sigma . w + m^2 \frac{dQ}{dz}, \quad \omega' = \Sigma . \omega + m^2 \frac{dQ}{dr}, \quad \sigma' = \Sigma . \sigma + m^2 \frac{dQ_1}{dz}.$$

From these results we may infer that, on proceeding to terms of the second order with respect to m , the composite velocities and condensations are not found to be equal to the *sums* of simple velocities and condensations, but to differ from such sums by small quantities of the second order involving the functions Q and Q_1 . Respecting these functions, it may be observed that they are wholly periodic, having as much positive as negative value.

But it is chiefly important to remark that the part of σ' which is expressed by $\Sigma . \sigma$ contains terms that do not change sign, viz.

$$\frac{m^2}{2a^2\kappa^4} \Sigma . [f^2 \sin^2 q\zeta] - \frac{m^2}{2a^2} \Sigma . \left[\frac{df^2}{q^2 dr^2} \cos^2 q\zeta \right],$$

and that the different terms which the symbol Σ embraces co-exist independently of each other. A distinction, however, should be made with respect to terms which have the same value of λ , but different values of c . For all such terms the

values of f and $\frac{df}{qdr}$ are the same; so that

$$\begin{aligned} (\Sigma . [f \sin q\zeta])^2 &= f^2 . (\Sigma . [\sin q(z - a't + c)])^2, \\ \left(\Sigma . \left[\frac{df}{qdr} \cos q\zeta \right] \right)^2 &= \frac{df^2}{q^2 dr^2} . (\Sigma . [\cos q(z - a't + c)])^2. \end{aligned}$$

But by a known trigonometrical theorem, $\Sigma . \left[\frac{\sin}{\cos} q(z - a't + c) \right]$ may take the form

$$(n + 2\Sigma . [\cos q(c_1 - c_2)])^{\frac{1}{2}} \frac{\sin}{\cos} q(z - a't + \theta),$$

n being the number of the terms, c_1 and c_2 any two values of c , and θ a function of the different values of c . Now, since the constant m may be supposed to be very small, and there is no limitation of the number n , and since also the values of c for *simple* vibrations do not admit of determination, it follows that there are no conditions by which the quantity $\Sigma . [\cos q(c_1 - c_2)]$ may be determined to be either positive or negative, and we may therefore suppose it to vanish. Hence if in the foregoing expressions for w' , ω' , and σ' , ζ be taken to represent $z - a't + \theta$, and if we introduce within the brackets $[]$ the factor $n^{\frac{1}{2}}$, those expressions will be adapted to the case in which the terms containing the same value of λ are *grouped* together. Consequently

the above expression for the part of σ' which does not change sign will become

$$\frac{m^2}{2a^2} \cdot \left\{ \Sigma \cdot \left[\frac{nf^2}{\kappa^4} \sin^2 q\zeta \right] - \Sigma \cdot \left[\frac{n}{q^2} \frac{df^2}{dr^2} \cos^2 q\zeta \right] \right\},$$

the constant θ being in general different for every different value of λ . It may here be remarked that, when the motion is compounded of simple vibrations having the same value of λ , the composite velocity to the first order of small quantities has, according to the foregoing reasoning, the value

$$mnf \sin \frac{2\pi}{\lambda} (z - a't + \theta);$$

and consequently, although m is an absolute constant, there may be vibrations of this type of all degrees of magnitude, the maximum velocities depending on different values of n .

Having thus determined how far the law of coexistence applies to vibrations relative to a single axis, we may proceed to consider similarly the coexistence of vibrations relative to *different* axes having any positions in space. Hitherto the axis of z has been supposed to coincide with the axis of the vibrations. Let us now conceive the origin and the directions of the rectangular coordinates to be any whatever, and let the coordinates of the previous investigation be transformed so as to be referred to the new origin and axes. After the transformation we shall still have $udx + vdy + wdz$ an exact differential, because this condition does not depend on the position of the axes of coordinates, but on the character of the motion. Hence if ψ_1 represent the transformed value of ψ , we shall also have, to the first order of small quantities,

$$\frac{d^2\psi_1}{dt^2} = a^2 \cdot \left(\frac{d^2\psi_1}{dx^2} + \frac{d^2\psi_1}{dy^2} + \frac{d^2\psi_1}{dz^2} \right).$$

Similarly, if ψ_2 be the transformed value of ψ for the vibrations relative to a different axis, we should have another such equation with ψ_2 in the place of ψ_1 , and so on to any number of sets of vibrations. Now since, from what is proved above, these different vibrations are such as may exist separately, it follows, from the equations being linear with constant coefficients, that they may exist conjointly. For if the equations be added, and ψ be put for $\psi_1 + \psi_2 + \psi_3 + \&c.$, it will be seen that the sum of the particular values $\psi_1, \psi_2, \&c.$ satisfies the same equation that they satisfy separately. After obtaining by this first approximation a composite value of ψ of explicit form, we might proceed to a second approximation by substituting this value in the terms of the equation (C) which are of the second order, it being still supposed that $(d\psi) = udx + vdy + wdz$. But from the

character of these terms it is evident that the small terms that are added by this operation will all be periodic, and have as much positive as negative value. The case will not, however, be the same with respect to the equation (B) after a like substitution. From what has just been said, the terms of the second order in the value of $\frac{d\psi}{dt}$ will be wholly periodic; but besides these there will be of the second order the terms

$$-\frac{a^2\sigma^2}{2} + \frac{1}{2} \left(\frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right).$$

Now, to the first approximation,

$$\frac{d^2\sigma}{dt^2} = a^2 \cdot \left(\frac{d^2\sigma}{dx^2} + \frac{d^2\sigma}{dy^2} + \frac{d^2\sigma}{dz^2} \right).$$

Hence, by the same reasoning as that applied to the function ψ , we shall have for different axes of vibrations,

$$\sigma = \sigma_1 + \sigma_2 + \sigma_3 + \&c.$$

Consequently

$$\sigma^2 = \sigma_1^2 + \sigma_2^2 + \sigma_3^2 + \&c. + \text{periodic terms,}$$

$$\frac{d\psi^2}{dx^2} = \frac{d\psi_1^2}{dx^2} + \frac{d\psi_2^2}{dx^2} + \frac{d\psi_3^2}{dx^2} + \&c. + \text{periodic terms,}$$

$$\&c. = \&c.$$

Hence, by substitution in the terms of the second order expressed above, it will be seen that the general value of σ will contain terms that are always positive, and that these terms are the *sums* of the terms that would be applicable to the several component vibrations if they existed separately. So far, therefore, as regards these condensations of the second order, the law of coexistence holds good, just as for those of the first order, and as independently of the relative positions of the axes of vibrations. This result, it is true, rests on the supposition that $udx + vdy + wdz$ is an exact differential to terms of the second order for vibrations about different axes; but since this is an *à priori* analytical hypothesis, the only inference to be drawn from this circumstance is, that the law is general with respect to vibratory motion, and independent of arbitrary conditions.

The law of the coexistence of vibrations having been thus ascertained to terms of the second order, we are prepared to form the equations applicable to given cases of motion in such manner as to take into account the general composite character of the motion of the fluid. With respect to this part of the investigation, I have seen no reason either to add to or correct what is contained in Parts II. and III., and shall therefore proceed to the third matter

that I proposed to consider in this communication (which is, in fact, the main object of it), viz. to ascertain the proper mode of applying the above-mentioned equations for determining the motion of a small sphere free to obey the impulses of the undulations of an elastic fluid. Here I shall have occasion to point out certain modifications of the reasoning in the latter portion of Part III.

The equations applicable to the present inquiry, so far as regards a *first* approximation, are those which are called (δ) and (ϵ) in Part II., viz.,

$$\left(\kappa^2 a^2 \cdot \frac{d\sigma}{dr} + \frac{dU}{dt}\right)U + \left(\kappa^2 a^2 \cdot \frac{d\sigma}{rd\theta} + \frac{dW}{dt}\right)W = 0, \quad (\delta)$$

$$\frac{d\sigma}{dt} + \frac{dU}{dr} + \frac{2U}{r} + \frac{dW}{rd\theta} + \frac{W}{r} \cot \theta = 0. \quad (\epsilon)$$

These equations embrace all cases in which the motion is symmetrical with respect to an axis, U and W being respectively the resolved parts of the velocity along and perpendicular to the radius vector r drawn from a point of the axis and inclined to it by the angle θ . Also to the first approximation the equations are the same, whether the origin of r be fixed or moving. Let us take the case of a disturbance of the fluid produced by a small smooth sphere vibrating with its centre always on a given straight line, and suppose the fluid to be incompressible and of unlimited extent. Since the sphere impresses motion only in the directions of the radii, if the motion be transmitted freely to an unlimited distance, the lines of motion at each instant are prolongations of the radii, and consequently $W=0$. This is also very nearly true for a compressible fluid of great elasticity if the radius of the sphere be very small. This solution of the problem of the simultaneous motions of a small sphere and the surrounding fluid I have elsewhere supported by arguments which it is unnecessary to cite here, my only reason for adverting to it being, that in former investigations I passed from this solution to the case of undulations impinging on a sphere at rest, by conceiving motions equal and opposite to those of the sphere to be impressed both on the sphere and on the fluid, and that I have recently been led to question the correctness of this principle. If the motion of the fluid relative to the sphere were actually the same in the two cases, it would follow that the motion produced by the reaction of the sphere at rest (which coexists with the rest of the motion) is such that the surfaces of displacement are concentric with the sphere, and the velocity at any point of a given surface varies as $\cos \theta$. Now although, as is indicated by the arguments above referred to, this may be true for a moving sphere, the lines of motion always diverging from the moving centre, and the motion of a *given* particle being consequently

curvilinear, it cannot be true when the lines of motion diverge from a fixed centre, because the motion in that case is rectilinear, and the velocity must consequently be a function of the distance from the centre.

The above-mentioned principle being thus shown to be inapplicable, the following process of solution has been adopted for the case of undulations incident on a *fixed* sphere. As in this case a relation exists between U and W which depends on the mutual action of the parts of the fluid, and is on that account to be ascertained by integration, the quantities in brackets in the equation (δ) are equated to zero, and from the two resulting equations and the equation (ϵ) U and W are eliminated. These operations conduct to the equation (ζ) in Part II., by differentiating which with respect to θ the equation employed in the solution of our problem is obtained, viz.

$$\frac{d^2P}{a'^2 dt^2} = \frac{d^2P}{dr^2} + \frac{1}{r^2} \left(\frac{d^2P}{d\theta^2} + \frac{dP}{d\theta} \cot \theta - \frac{P}{\sin^2 \theta} \right), \quad . \quad . \quad (\eta)$$

P being put for $r \frac{d\sigma}{d\theta}$, and a' for κa , for the sake of brevity. This equation is to be used rather than the one from which it was immediately deduced, because, while it includes the latter, it admits of more extensive application, as will appear in the sequel. By supposing that $P = \phi_1 \sin \theta + \phi_2 \sin \theta \cos \theta$, it is found that the equation is satisfied if ϕ_1 and ϕ_2 be functions of r and t determined by integrating the equations .

$$\begin{aligned} \frac{d^2\phi_1}{a'^2 dt^2} - \frac{d^2\phi_1}{dr^2} + \frac{2\phi_1}{r^2} &= 0, \\ \frac{d^2\phi_2}{a'^2 dt^2} - \frac{d^2\phi_2}{dr^2} + \frac{6\phi_2}{r^3} &= 0. \end{aligned}$$

Now, if all the conditions of the problem can be satisfied by this particular integration, it will follow that we have obtained the only appropriate value of P , and that no other solution of the problem is possible. It will be now assumed that the incident undulations are defined by the equations

$$V = a' \sigma' = m' \sin q(a't + r \cos \theta + c_0),$$

and that the direction of incidence is *contrary* to that in which the radius vector is drawn when $\theta = 0$. As in the applications proposed to be made of these researches the radius of the sphere will always be extremely small, it will be supposed that, while the distance r_1 from the centre of the sphere within which its reaction on the fluid is of sensible magnitude is very large compared to c the radius of the sphere, it is very small compared to

λ the breadth of an undulation; so that $\frac{c}{r_1} \times \frac{r_1}{\lambda}$, or $\frac{c}{\lambda}$, is a small quantity of the second order. Hence, since qr may on this supposition be always taken to be very small, it is allowable to expand the above sine in terms proceeding according to the powers of this quantity. We shall thus have, to terms of the second order,

$$V = a't' = m' \sin q(a't + c_0) + m' \cos q(a't + c_0)qr \cos \theta \\ - \frac{m'}{2} \sin q(a't + c_0)q^2 r^2 \cos^2 \theta.$$

The sole conditions that the integration of the equation (η) is required to fulfil are, (1) that these approximate equations be satisfied wherever r is very large compared to c and very small compared to λ ; (2) that $U=0$ where $r=c$,—that is, at the surface of the sphere. Again, since the equation (η) is verified by supposing P to be either $\phi_1 \sin \theta$, or $\phi_2 \sin \theta \cos \theta$, or the sum of these two quantities, let us first suppose $P = \phi_1 \sin \theta$. Then, as is shown in Part II., the following equations may be obtained,

$$\sigma = \psi(r, t) - \left(\frac{f+F}{r^2} - \frac{f'+F'}{r} \right) \cos \theta,$$

$$\frac{W}{a'} = \left(\frac{f_1 - F_1}{r^3} - \frac{f - F}{r^2} \right) \sin \theta,$$

$$\frac{U}{a'} = \left(\frac{2(f_1 - F_1)}{r^3} - \frac{2(f - F)}{r^2} + \frac{f' - F'}{r} \right) \cos \theta,$$

f, f', f_1 being put respectively for $f(r - a't)$, $\frac{df}{dr}$, and $\int f dr$, and

F, F', F_1 for $F(r + a't)$, $\frac{dF}{dr}$, and $\int F dr$. Since from the condi-

tions of the problem no part of σ can be a function of r without θ , the arbitrary quantity $\psi(r, t)$ cannot contain r , but must be a function of t only. To determine this function, let $\theta = \frac{\pi}{2}$.

Then, for all the corresponding values of r , $\sigma = \psi(t) = \sigma_1$ suppose. But for the large values of r , σ_1 is equal to what σ' becomes when

$\frac{\pi}{2}$ is put for θ . Hence $\sigma_1 = \frac{m'}{a'} \sin q(a't + c_0)$.

As the forms of the functions f and F depend entirely on that of the function which expresses the law of the velocity and condensation of the incident undulations, it will be assumed that

$$f = m_1 \sin q(r - a't + c_1), \quad F = m_2 \sin q(r + a't + c_2).$$

By the condition that $U=0$ where $r=c$, we have

$$\frac{2(f_1 - F_1)}{c^2} - \frac{2(f - F)}{c} + f' - F' = 0.$$

After substituting in this equation the above values of f and F and putting c for r , it will be found that the equation is satisfied for all values of t if $m_2 = -m_1$ and $c_2 = c_1$, and if the arbitrary constant c_1 is determined by the equation

$$\tan q(c + c_1) = -\frac{1}{qc} + \frac{qc}{2}.$$

Also if we take another set of values of f and F , distinguishing them from the preceding by dashes attached to the constants, the same equation will be satisfied if $m'_2 = m'_1$ and $c'_2 = c'_1$, and if c'_1 be determined by the equation

$$\cot q(c + c'_1) = \frac{1}{qc} - \frac{qc}{2}.$$

As these two methods of satisfying the condition $U=0$ are equally entitled to consideration, *both* must be employed in deducing the value of σ . Here it is to be remarked that, on account of the linear form of the differential equation from which ϕ_1 is obtained, we might have Σf and ΣF in place of f and F . This being the case, it is allowable to substitute in the expression for $\sigma - \sigma_1$ the respective sums of the two values of f and F . When this is done, and the relations between the constants are taken into account, the result is

$$\begin{aligned} \sigma - \sigma_1 = & \left(\frac{2m_1}{r^2} \cos q(r + c_1) + \frac{2m_1 q}{r} \sin q(r + c_1) \right) \sin qa't \cos \theta \\ & - \left(\frac{2m'_1}{r^2} \sin q(r + c'_1) - \frac{2m'_1 q}{r} \cos q(r + c'_1) \right) \cos qa't \cos \theta. \end{aligned}$$

At the same time the foregoing equations for finding c_1 and c'_1 give very approximately

$$\cos qc_1 = -\frac{q^3 c^3}{6}, \quad \sin qc_1 = 1; \quad \cos qc'_1 = 1, \quad \sin qc'_1 = \frac{q^3 c^3}{6}.$$

By having regard to these values of c_1 and c'_1 , expanding the sines and cosines, and omitting insignificant terms, the above equation becomes

$$\sigma - \sigma_1 = - \left(\frac{q^3 c^3}{3r^2} + \frac{2q^3 r}{3} \right) (m_1 \sin qa't + m'_1 \cos qa't) \cos \theta.$$

When r is very large compared to c , the first term in the first brackets may be omitted, and the consequent value of $\sigma - \sigma_1$ must then satisfy the condition of being identical with the term

containing $\cos \theta$ in the value of $\sigma' - \sigma_1$. Hence

$$-\frac{2q^2}{3}(m_1 \sin qa't + m'_1 \cos qa't) = \frac{m'}{a'} \cos q(a't + c_0),$$

$$m_1 = \frac{3m'}{2q^2 a'} \sin qc_0, \quad m'_1 = -\frac{3m'}{2q^2 a'} \cos qc_0.$$

Substituting these values of m_1 and m'_1 and putting c for r , we have finally, for the condensation at any point of the surface of the sphere,

$$\sigma = \sigma_1 + \frac{3qc}{2a'} m' \cos q(a't + c_0) \cos \theta.$$

Also from the equation $\frac{a'^2 d\sigma}{cd\theta} + \frac{dW}{dt} = 0$ we find, for the velocity along the surface,

$$W = \frac{3m'}{2} \sin q(a't + c_0) \sin \theta.$$

Since, as has been already remarked, the differential equations to this order of approximation are the same whether the origin of the polar coordinates be fixed or moving, the preceding results may be made applicable to the case of a small sphere oscillating in the fluid at rest. In fact, if the vibrations of the fluid be counteracted by impressing equal and opposite vibrations, and if the same vibrations be impressed on the sphere, we pass from the problem above considered to that of the oscillating sphere. But in consequence of these impressed velocities W is evidently diminished by $m' \sin q(a't + c_0) \sin \theta$, and σ is diminished by the condensation due to the state of vibration of the incident fluid—

that is, by $\sigma_1 + \frac{d\sigma_1}{a'dt} c \cos \theta$. After subtracting these quantities,

the remaining values of σ and W are precisely those which are obtained by the usual mode of solving the problem of the disturbance of the fluid by the sphere. Now it is to be observed that in the case of each problem the velocities and pressures are the same, as far as regards approximations of the first degree, whether the fluid be considered very elastic or wholly incompressible. But if the fluid be incompressible, its action on the sphere at rest will be the same as if a cubical portion of it, rigidly separated from the rest, and having the sphere near its centre, were compelled to oscillate bodily in a direction perpendicular to a separating plane, provided always the rigid boundary be far beyond the limit of any sensible effect of the sphere's reaction. It is evident that under these circumstances as much fluid passes a plane through the centre of the sphere perpendicular to the direction of incidence as would have passed if the sphere had not been there, which is exactly what the preceding analysis indicates. Con-

versely, if the cubical portion of fluid be stationary and the sphere oscillate, the fluid which passes the plane through the sphere's centre must be just as much as the sphere displaces; and this, again, results from the usual analytical treatment of this problem. But from these considerations it necessarily follows that the condition of the rigid boundary is involved in that mode of treatment. By reasoning contained in this and previous communications, I have pointed out that this condition is implicitly introduced when $udx + vdy + wdz$ is assumed to be an exact differential for this instance of arbitrary disturbance. The two problems here considered admit of that assumption, and are on that account *correlative*: but the problem of the sphere oscillating in *unlimited* fluid is distinct from these, not being a case for which $udx + vdy + wdz$ is integrable without a factor, and requiring on that account particular treatment, which I have already frequently discussed and need not further advert to here.

Proceeding now to trace the consequences of supposing that in the equation (η) P is equal to $\phi_2 \sin \theta \cos \theta$, we have first the following results obtained by the process indicated in Part II., viz.

$$\sigma = \sigma_1 - \left(\frac{f+F}{r^3} - \frac{f'+F'}{r^2} + \frac{f''+F''}{3r} \right) \frac{\cos^2 \theta}{2},$$

$$\frac{W}{a'} = \left(\frac{f_1-F_1}{r^4} - \frac{f-F}{r^3} + \frac{f'-F'}{3r^2} \right) \sin \theta \cos \theta,$$

$$\frac{U}{a'} = \left(\frac{3(f_1-F_1)}{r^4} - \frac{3(f-F)}{r^3} + \frac{4(f'-F')}{3r^2} - \frac{f''-F''}{3r} \right) \frac{\cos^2 \theta}{2}.$$

As this integration is independent of the previous one, f and F may be supposed to have new values. Also for the same reason $U=0$ where $r=c$, without respect to the former value of U . Since it may be presumed from the former integration that two sets of values of f and F will be required to satisfy the given conditions, let us suppose that

$$f = m_3 \sin q(r - a't + c_3) + m'_3 \sin q(r - a't + c'_3),$$

$$F = m_4 \sin q(r + a't + c_4) + m'_4 \sin q(r + a't + c'_4).$$

On substituting these functions in the expression for U , it will be found that the condition, $U=0$ where $r=c$, is satisfied if $m_4 = -m_3$, $c_4 = c_3$, $m'_4 = m'_3$, $c'_4 = c'_3$, and if c_3 and c'_3 be determined by the following equations,

$$\tan q(c + c_3) = -\frac{1}{qc} \cdot \frac{9 - 4q^2 c^2}{9 - q^2 c^2}, \quad \tan q(c + c'_3) = qc \cdot \frac{9 - q^2 c^2}{9 - 4q^2 c^2}.$$

These equations give very approximately

$$\sin qc_3 = 1, \quad \cos qc_3 = -\frac{2q^5c^5}{135}, \quad \sin qc'_3 = \frac{2q^5c^5}{135}, \quad \cos qc'_3 = 1.$$

By substituting in the expression for σ the supposed values of f and F , and taking account of the relations between the constants, it will be found that

$$\begin{aligned} \sigma - \sigma_1 = & \left(\frac{q}{r^2} \sin q(r+c_3) + \left(\frac{1}{r^3} - \frac{q^2}{3r} \right) \cos q(r+c_3) \right) m_3 \cos qa't \cos^2 \theta \\ & + \left(\frac{q}{r^2} \cos q(r+c'_3) - \left(\frac{1}{r^3} - \frac{q^2}{3r} \right) \sin q(r+c'_3) \right) m'_3 \cos qa't \cos^2 \theta. \end{aligned}$$

After eliminating c_3 and c'_3 by the equations above, expanding the sines and cosines of qr , and neglecting insignificant terms, this equation is reduced to the following:—

$$\sigma - \sigma_1 = - \left(\frac{q^5r^2}{45} + \frac{2q^5c^5}{135r^3} \right) (m_3 \sin qa't + m'_3 \cos qa't) \cos^2 \theta.$$

Then by supposing r to be very much larger than c and neglecting the second term in the first brackets, and by equating the result to the term of $\sigma' - \sigma_1$ which contains $\cos^2 \theta$, we obtain

$$\begin{aligned} \frac{m'}{2a'} \sin q(a't + c_0) &= \frac{q^3}{45} (m_3 \sin qa't + m'_3 \cos qa't), \\ m_3 &= \frac{45m' \cos qc_0}{2q^3a'}, \quad m'_3 = \frac{45m' \sin qc_0}{2q^3a'}. \end{aligned}$$

Hence, substituting these values of m_3 and m'_3 in the foregoing equation, and putting c for r , we have for the condensation at any point of the surface of the sphere,

$$\sigma = \sigma_1 - \frac{5m'q^2c^2}{6a'} \sin q(a't + c_0) \cos^2 \theta.$$

The velocity along the surface deduced from this value of σ is

$$\frac{5m'qc}{3} \cos q(a't + c_0) \sin \theta \cos \theta.$$

Adding now the results of the two integrations, and using σ and W to represent the total condensation and velocity at any point of the surface, we have

$$\begin{aligned} \sigma - \sigma_1 &= \frac{3qcm'}{2a'} \cos q(a't + c_0) \cos \theta - \frac{5m'q^2c^2}{6a'} \sin q(a't + c_0) \cos^2 \theta, \\ W &= \frac{3m'}{2} \sin q(a't + c_0) \sin \theta + \frac{5m'qc}{6a'} \cos q(a't + c_0) \sin 2\theta. \end{aligned}$$

The resultant of the pressures at all points of the surface, estimated in the direction of the incidence of the vibrations, is

$$2\pi c^2 \int a^2(\sigma - \sigma_1) d\theta \sin \theta \cos \theta,$$

taken from $\theta=0$ to $\theta=\pi$. Between these limits the integral relative to the term containing $\cos^2 \theta$ is evidently zero, and the resultant pressure is consequently

$$\frac{2\pi m' q a^2 c^3}{a'} \cos q(a't + c_0).$$

Hence, supposing the mass of the sphere to be $\frac{4\pi c^3 \Delta}{3}$, the accelerative action of the fluid on the sphere is

$$\frac{3m' q a^2}{2\Delta a'} \cos q(a't + c_0),$$

which, it may be remarked, is independent of the magnitude of the sphere.

In order to complete the determination of the resultant of the pressure on the sphere, other considerations have now to be entered upon, from which it will appear that the action of the fluid on the second hemispherical surface is not, as the reasoning so far indicates, of exactly the same amount as that on the first. The circumstance which modifies the pressure on the second hemisphere is the composite character of the incident vibrations, in consequence of which, as soon as they are propagated beyond the first hemisphere and direct incidence ceases, the transverse vibrations come into play, being no longer neutralized. The consideration of this particular action of the fluid is an essential part of my Theory of the Dispersion of Light, and I shall therefore have occasion here to say little more than what is contained in the Supplementary Number of the Philosophical Magazine for December 1864. In the extreme case of vibrations so rapid that the value of λ is small compared to the radius of the sphere, the transverse action might have the effect of preventing the undulations extending more than a short distance along the second hemisphere, so that the fluid in contact with the greater part of its surface would be at rest. This is not the case of the present problem, because c has been assumed to be extremely small compared to λ , and the excursion of a given particle of the fluid may be supposed to be equal to, or even much larger than, the radius of the sphere. But in all cases the transverse action will cause the state of the fluid on the further side of the plane through the sphere's centre to differ from that on the other, and the calculation of the exact amount of the difference should be within the reach of analysis. This part of the problem, however, which

requires the application of new principles, I do not profess to have solved. I have elsewhere stated that motion compounded of direct and transverse vibrations relative to an unlimited number of parallel axes might be such as to be contained within a cylindrical surface of small radius; but I have not attempted to determine the law of the diminution of the motion at parts contiguous to the boundary. For this reason I am not prepared to determine the effect of the transverse action in the present problem so as to obtain expressions for the velocity and condensation at all positions beyond the plane through the centre of the sphere. Still it is possible to arrive at certain results of much theoretical importance, which I now proceed to develop.

Considering only positions contiguous to the second hemispherical surface, the state of the fluid there is required to fulfil the following three conditions: (1) it must be such as to satisfy the equation (η); (2) the motion being wholly along the surface, $U=0$; (3) as the transverse action affects the amount but not the *phase* of the condensation, the change of condensation it produces at a given point varies as σ_1 , or $m' \sin q(a't + c_0)$. Now these conditions may all be fulfilled by means of the foregoing integration involving the function ϕ_2 , if only the arbitrary constants be differently determined. For by that integration we may have

$$\sigma - \sigma_1 = - \left(\frac{q^5 r^2}{45} + \frac{2q^5 c^5}{135r^3} \right) (m_5 \sin q a't + m'_5 \cos q a't) \cos^2 \theta.$$

Hence $\frac{d\sigma}{dr} = 0$, and consequently $U=0$, where $r=c$. Let

$m_5 = h m_3$ and $m'_5 = h m'_3$, m_3 and m'_3 being the former values of the constants, and h being an unknown constant depending on the transverse action. Then putting c for r , substituting the values of m_3 and m'_3 , and subtracting the resulting equation from the former value of $\sigma - \sigma_1$, we shall obtain, for the change of condensation produced at the second hemispherical surface by the transverse action,

$$- \frac{5q^2 c^2}{6a'} (1-h) m' \sin q(a't + c_0) \cos^2 \theta,$$

which satisfies the third condition. This expression vanishes if $\theta = \frac{\pi}{2}$, as also does the corresponding expression for the velocity.

The whole pressure on the sphere due to this condensation and estimated in the direction of propagation, will be found, by integrating from $\theta = \frac{\pi}{2}$ to $\theta = \pi$, to be

$$\frac{5\pi q^2 c^4 a^2}{12a'} (1-h) m' \sin q(a't + c_0).$$

Hence, adding this to the former determination of the pressure, it will follow that the total accelerative action on the sphere is

$$\frac{3qa^2}{2\Delta a'} m' \cos q(a't + c_0) + \frac{5q^2ca^2}{16\Delta a'} (1-h) m' \sin q(a't + c_0).$$

Hitherto the sphere has been supposed to be fixed. But the object of this inquiry is to determine the motion of a sphere free to obey the impulses of the fluid. To make the preceding investigation applicable to the solution of this problem, I adopt the principle that the action of the undulations on the sphere in motion is due to the *relative* motion of the fluid and sphere. Let x be the distance of the centre of the sphere at any time t from an arbitrary origin, and be reckoned positive in the direction of incidence, and let the excess of the velocity of the fluid be

$$m \sin q(a't - x + \alpha) - \frac{dx}{dt}.$$

According to the above principle, this quantity is the equivalent of $m' \sin q(a't + c_0)$ in the former reasoning. The centre of the sphere being supposed to perform small oscillations about a mean position, if for x within the brackets we substitute its mean value, only quantities of the second order will be omitted. Hence, putting C for $-x + \alpha$, we have

$$m' \sin q(a't + c_0) = m \sin q(a't + C) - \frac{dx}{dt};$$

and by differentiating,

$$m' q a' \cos q(a't + c_0) = m q a' \cos q(a't + C) - \frac{d^2x}{dt^2}.$$

In this equation the foregoing expression for the accelerative force impressed on the fixed sphere is to be substituted for $\frac{d^2x}{dt^2}$, and the equation is then to be identically satisfied. These operations conduct to the following equalities, $q\omega$ being an auxiliary arc:—

$$\tan q\omega = \frac{5qc(1-h)}{8(3+2\Delta\kappa^2)}, \quad c_0 = C - \omega, \quad \frac{m'}{m} = \frac{2\Delta\kappa^2 \cos q\omega}{3+2\Delta\kappa^2}.$$

If now by means of these relations we eliminate m' and c_0 from the expression for $\frac{d^2x}{dt^2}$, and neglect terms involving the square of $q\omega$, this being a very small arc the result will be

$$\frac{d^2x}{dt^2} = \frac{3a'q}{3+2\Delta\kappa^2} m \cos q(a't + C) + \frac{5q^2ca'(3+\Delta\kappa^2)}{4(3+2\Delta\kappa^2)^2} (1-h) m \sin q(a't + C).$$

It is here to be remarked that since the factor $1-h$ has relation,

not to difference of velocity, but to difference of condensation, the term containing it must involve the condensation. If, therefore, $V = a'S = m \sin q(a't + C)$, and if H and K be substituted for numerical coefficients the values of which are known if Δ be given, we have finally

$$\frac{d^2x}{dt^2} = H \cdot \frac{dV}{dt} + Kq^2c(1-h)a^2S.$$

The acceleration of the sphere has thus been determined, so far as it depends on the terms in the original values of σ and w which contain the first power of m ; and it will be seen that the above expression for it is wholly periodic. From this first approximation we might next proceed to include terms involving m^2 . But since these terms are of very small magnitude compared to those which have been considered, we may dispense with going through the details of the second approximation by making use of a general analytical formula, according to which if $f(Q)$ be a first approximation to an unknown function of any quantity Q , the second approximation is $f(Q) + f'(Q)\Delta Q$, if ΔQ be very small. By applying this formula to each term of the above expression for $\frac{d^2x}{dt^2}$, we have to the second approximation very nearly

$$\frac{d^2x}{dt^2} = H \cdot \left(\frac{dV}{dt} + \frac{d \cdot \Delta V}{dt} \right) + Kq^2c(1-h)(S + \Delta S).$$

Now, from what was proved relative to the component vibrations, ΔV is wholly periodic, and so, by consequence, is $\frac{d \cdot \Delta V}{dt}$; but it was shown that ΔS contains terms that are *always positive*. These terms consequently indicate that the sphere has a *permanent motion of translation*. This motion is towards or from the origin of the undulations according as h is greater or less than unity.

Again, it was proved that different sets of such positive terms, corresponding to different origins of disturbance, may coexist; from which it follows that simultaneous undulations from different sources may *independently produce motions of translation of the sphere*.

It also appeared that when the axes of the component vibrations are exactly or very nearly parallel, the sum of the condensations expressed by these positive terms is proportional to the number of the axes that pass through a given area. Hence, if the axes diverge from a centre, this sum, and, by consequence, the corresponding accelerative action of the undulations, *varies inversely as the square of the distance from the centre*.

Without knowing the composition of the constant h , it is possible to point to circumstances under which its value may exceed unity. For suppose m and λ for the incident vibrations to be very large; then as soon as the transverse vibrations are brought into action on the further side of the sphere, the motion of the fluid will partake of the character of direct and transverse vibrations relative to an axis, the axis in this case being the prolongation of a straight line through the centre of the sphere in the direction of propagation. But for motion of that kind it has been shown that the transverse vibrations have the effect of increasing the condensation on the axis, compared with that for the same velocity when the motion is in parallel lines, in the ratio of κ^2 to 1. By a like transverse action the condensation on the further side of the sphere might be so increased as to exceed that on the nearer side; in which case h would be greater than unity, and the sphere would be drawn towards the origin of the waves, being acted upon as by an *attractive force*. On the contrary, for very small values of m and λ the defect of condensation on the further side of the sphere might be only partially supplied by the lateral confluence, so that h would be less than unity, and the translation of the sphere would be from the origin of the waves, their action resembling that of a *repulsive force*. In the Theory of Dispersion the term involving $1-h$ had to be neglected, probably because for luminous undulations $h=1$ nearly, and the translating power is very feeble.

I do not purpose at present to discuss further the inferences that may be drawn from the above determination of the acceleration of the sphere, and shall only remark, in conclusion, that although the principles of the present solution of this problem are fundamentally the same that I have previously employed, I consider the proofs of the preliminary propositions to be more exact, and the details of the general argument to be more logically arranged than in former attempts. As I have already said, this communication professes to contain nothing but mathematical reasoning based on admitted premises, and both the reasoning and the results may be regarded quite apart from any ulterior physical applications of them. I cannot, however, but be of the opinion that if the undulations of an elastic fluid can be proved to be capable of causing permanent motions of translation, this fact must materially influence the view that we take of the nature of the physical forces.

Cambridge, April 11, 1866.

LII. *On the Galvanometer Resistance to be employed in testing with Wheatstone's Diagram.* By LOUIS SCHWENDLER, *Electrician to Siemens Brothers, Woolwich*.*

IT has been proved elsewhere that, in measuring resistances with Wheatstone's diagram, the highest degree of sensitiveness is attained when the four branch resistances are equal; but no investigation exists at present, to my knowledge, for determining the equally important question,

Which relative galvanometer resistance raises the magnetic moment of the galvanometer to a maximum when the other branches of the diagram are given.

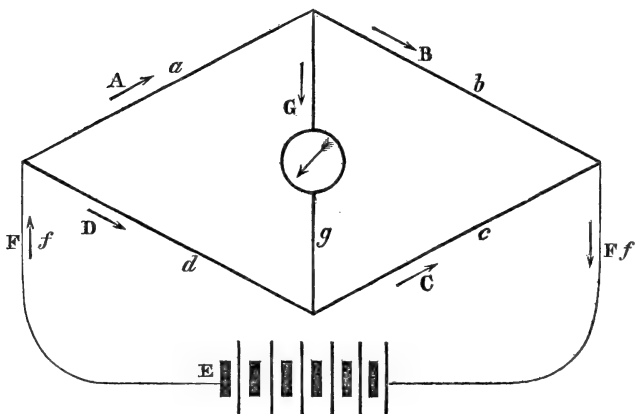
It has been further proved that, in order to raise the magnetic moment of the galvanometer in simple circuit to a maximum, the resistance of the galvanometer must be equal to the external resistance.

For analogous reasons, it may be inferred that there must be a similar law in measuring resistances with Wheatstone's diagram; and the only question is, Which in this case is the external resistance?

An inquiry into this question is the purpose of the present paper.

In fig. 1 let a , b , c , and d represent the resistances of the four branches, f that in battery branch, and g the unknown resistance of the galvanometer. Now, if we call A, B, C, D, F, and G re-

Fig. 1.



spectively the current in each of the six different branches, and E the electromotive force, we have, according to the two laws of

* Communicated by the Author.

Kirchhoff, the following six equations, which are independent of one another:—

$$A = B + G,$$

$$C = D + G,$$

$$F = A + D,$$

$$aA + gG - dD = 0,$$

$$fF + aA + gG + cC = E,$$

and
$$gG + cC - bB = 0.$$

By eliminating A , B , C , D , and F we obtain a single equation containing the electromotive force E , the current G in the galvanometer, and the six resistances a , b , c , d , g , and f . If we now develop G from this equation, we have

$$G = \frac{E}{g \frac{(c+d)(a+b) + f(a+b+c+d)}{bd-ac} + f \frac{(b+c)(a+d) + ab(c+d) + cd(a+b)}{bd-ac}};$$

by putting $bd - ac = 0$, we have $G = 0$, which is the known law of Wheatstone's diagram.

Let us substitute

$$(c+d)(a+b) + f(a+b+c+d) = V,$$

$$bd - ac = \alpha,$$

$$f(b+c)(a+d) + ab(c+d) + cd(a+b) = W,$$

and multiply the equation by U , the number of turns in the galvanometer-coil, we obtain its magnetic moment, which may be called Y ,

$$Y = E\alpha \frac{U}{gV + W}.$$

The space to be filled with wire of known and constant conductivity being given, and calling q the sectional area of this wire, we have between U , q , and g the following two equations:—

$$U = \frac{\text{const.}}{q},$$

$$g = \frac{U \text{ const.}}{q};$$

or therefore

$$U = \text{const.} \sqrt{g},$$

and consequently

$$Y = E\alpha \cdot \frac{\sqrt{g}}{gV + W} \cdot \text{const.} = E\alpha \frac{\sqrt{g}}{gV + W}.$$

In this equation only g is taken as variable; and the question now is, what must be the value of g in order to raise Y to its maximum.

This will be the case when $g = \frac{W}{V}$, or, by substituting the values of W and V ,

$$g = \frac{f(b+c)(a+d) + ab(c+d) + cd(a+b)}{(c+d)(a+b) + f(a+b+c+d)};$$

by dividing both numerator and denominator by ac , we obtain an equation better suited to our purpose,

$$g = \frac{f\left(1 + \frac{d}{a}\right)\left(1 + \frac{b}{c}\right) + bd\left(\frac{1}{a} + \frac{1}{c}\right) + b + d}{\left(1 + \frac{d}{c}\right)\left(1 + \frac{b}{a}\right) + f\left(\frac{b+d}{ac} + \frac{1}{a} + \frac{1}{c}\right)} \quad \dots (1)$$

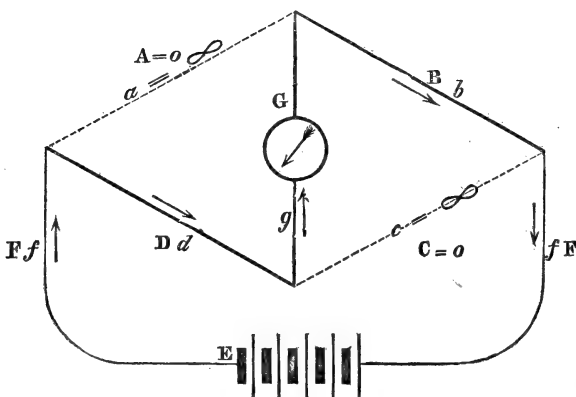
This equation (1) establishes a general dependence between the resistance of the galvanometer and that of the other five branches, including the battery branch when the magnetic moment of the galvanometer-coil is a maximum. The above equation having been developed without reference to a balance, we must obtain the known law of simple circuit if we put

$$a = c = \infty.$$

Fig. 1 is then changed into fig. 2, and equation (1) gives us

$$g = f + b + d,$$

Fig. 2.



which is the law for simple circuit, proving the general applicability of this equation.

Instead of equation (1) we may put

$$g = \frac{ab}{a+b} + \frac{cd}{c+d} + f\gamma, \quad (1)$$

which is identical with the other if

$$\gamma = \frac{(ac-bd)^2}{(a+b)(c+d) \{ (a+b)(c+d) + f(a+b+c+d) \}}.$$

But as it is evident that the law (1) is of no practical interest unless balance is almost established, we have always $ac-bd$ very near zero; consequently γ , which is proportional to the square of $ab-cd$, must approximate still nearer to zero; and f , the resistance in the battery branch, being already as small as possible, we may in this case, without sensible error, put

$$f\gamma = 0;$$

hence we have approximately

$$g = \frac{ab}{a+b} + \frac{cd}{c+d}. \quad (1)$$

But as $(ac-bd)^2$ is infinitely small, we may write

$$"g = \frac{(a+d)(b+c)}{a+b+c+d}." \quad (2)$$

This equation gives us the following simple law for Wheatstone's diagram when near a balance:—

To raise the magnetic moment of the galvanometer to its maximum, its resistance must be equal to the parallel resistance of the two double branches which are opposite to the galvanometer.*

And according to this, we can always calculate the proper resistance of the galvanometer, viz. the diameter of wire to be used when the space to be filled is given.

The above law gives us, for every other resistance to be measured, another resistance of the galvanometer; but as the galvanometer can only have a very limited number of definite resistances, the law we have given must apply to those values to be measured for which the other conditions are the most unfavourable; and it is evident this will be the case for all large resist-

* I will mention here that this law is only correct when the sectional area of the insulating covering is always in the same proportion to g , the sectional area of the wire. But as the thickness of the insulating covering is generally the same for thick as for thin wires, this condition is never fulfilled in practice, especially with small galvanometers used in measuring large resistances.

ances, which can be measured only with a great difference in the branches a and d .

With the usual bridges generally applied in cable-testing, we can measure resistances between $\frac{1}{100}$ –1,000,000 Siemens's units, having in the branches both a and d the resistances 10, 100, and 1000; and in the comparison-coil, resistances variable from 1–10,000 Siemens's units; and it is undeniable that, having to choose the resistance of a galvanometer with one coil only for this kind of bridge, our law must apply to all resistances between 100,000 and 1,000,000 Siemens's units, or to the average resistance 550,000.

To measure 550,000 Siemens's units, we have to put in equation (2)

$$a=10,$$

$$d=1000,$$

$$b=5500,$$

and

$$c=550000;$$

and then we have

$$g=1009 \text{ Siemens's units.}$$

For practical use it will be better to write equation (2) in the following form,

$$g = \left(\frac{a+d}{c+d} \right) c, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which is identical with the other, because $ac-bd$ is infinitely small.

If c , the resistance to be measured, is very large in proportion to the largest branch d , we can neglect d against c , and therefore we have approximately

$$“g=a+d;”$$

or, in this case,

To raise the magnetic moment of a galvanometer to its maximum, its resistance must be equal to the sum of the two smallest branches.

[In this case it will be necessary to add a correction to equation (2), which I intend to give in a subsequent article.]

LIII. *On the Completion of the Demonstration of Newton's Rule, and on a general property of derived Polynomials.* By J. R. YOUNG, formerly Professor of Mathematics in Belfast College*.

PRESUMING that the readers of the present communication are familiar with my papers on Newton's Rule in the Magazines for August and October last, I now proceed to supply what appears to be necessary in order to complete the demonstration of that rule. And first I observe that whenever Newton's condition of imaginary roots holds for any triad of terms in a primitive equation, it necessarily holds also in every equation derived from it into which the same triad of coefficients enters; and consequently it holds for every reciprocal of these derived equations.

This is a circumstance peculiar to the class of imaginaries thus indicated; and from which it follows that the cubics derived from the reciprocal equations will retain among them just as many several conditions of imaginary roots as are presented to us in the primitive equation. This truth is sufficiently established by the series of cubics, in a general form, deduced from the equally general reciprocal equations.

Taking these cubics in order, from the first to the last, we see that the entrance of an imaginary pair into an advanced cubic may be necessitated by, and therefore dependent on, the entrance of an imaginary pair into the preceding cubic just passed over; and that this again may necessitate the entrance of a pair into the next succeeding cubic, and so on.

Suppose, however, that these dependencies cease, and that we at length arrive at a cubic the imaginary pair in which is underrived from, and independent of, preceding pairs. Would such an occurrence warrant the inference that there must be at least two pairs of imaginary roots in the primitive equation? That it would, is the principle assumed in my early investigation of Newton's Rule, it having presented itself to me at the time as an axiomatic truth: it may be proved to be a truth as follows:—

Let there be taken the cubic (C') in which the imaginary pair does *not* derive its character, *as* an imaginary pair, from any preceding cubic, and *also* one of these preceding cubics (C) itself; then representing this preceding imaginary pair by I , and the other pair by I' , we know that the entrance of I' is not a consequence of the entrance of I , but is entirely independent of the entrance of I .

Calling the two reciprocal equations from which the cubics in question (C , C') have been derived R , R' , we may, by reversing the process by which C , C' were deduced from R , R' , deduce

* Communicated by the Author.

these latter from the former, adding in, at each reverse step, that particular constant (or final term) which, in the direct step, was made to disappear.

In the equation of the fourth degree (the result of the first step from C) there enters an imaginary pair necessarily and exclusively dependent for its imaginary character on the pair in C. But the pair in C' is *not* dependent on this pair in the biquadratic; if it were, it would be dependent on the pair in C, which it is not. In like manner, the equation of the fifth degree in the next reverse step has an imaginary pair dependent on the before-mentioned pair in the preceding result, and therefore on the pair in C. The pair in C' is therefore equally independent of *this* pair, and so on through all the reverse steps up to R; that is, there is a pair of imaginary roots in R of which the imaginary pair in the cubic C', derived from R', is independent. But imaginary roots can enter R' only as a consequence of imaginary roots entering R; and imaginary roots can enter C' only as a consequence of imaginary roots entering R', and *therefore* only as a consequence of imaginary roots entering R. But it has been shown that the imaginaries in C' do not enter as a consequence of that particular pair in R of which the pair in C (in the reverse process of derivation) is the source; hence the pair in C' must be the consequence of some *other* pair in R, which therefore has at least *two* pairs of imaginary roots. Consequently the primitive equation has at least two pairs of imaginary roots.

In the foregoing reasoning R is regarded as derived from C (after restoring the factor previously expunged) by the process of integration; and it is to be observed that it is the *general* integral at each step that owes a pair of imaginary roots to the entrance of a pair into C,—in other words, that though the constant completing any integral be quite arbitrary, or even zero, a pair of imaginary roots, resulting from the pair in C, must still enter. The constants actually introduced are each of assigned value, as a specific equation (R) with assigned coefficients is to be deduced: these constants, added one after another as the derivation proceeds, may cause the introduction of additional imaginary pairs into the successive equations of even degree; but none of these pairs are traceable to the pair in C, and a pair of imaginaries traceable to those in C would still enter each of the ascending equations, though no constants at all were introduced.

I have felt anxious that my demonstration of Newton's Rule should be thus rendered complete, not because I regard the Rule itself as of much practical importance in the solution of numerical equations, but because I have always thought the line of

argument I have adopted to be very similar to that which Newton himself must have employed: I trust that there is no unpardonable presumption in my thus thinking. Newton and his contemporaries—more especially Maclaurin—paid much attention to, and made much use, in their algebraical investigations, of, *limiting* equations, of which they gave many varieties of form. It is not likely that Newton had anticipated any of the peculiar views of Fourier; it is more probable that he arrived at his Rule, as I have arrived at it, solely from the consideration of limiting equations, and that his investigation of it was suppressed on account of its length. In reference to his researches on the Doctrine of Equations, in his 'Universal Arithmetic,' he says (Raphson's Translation, 1720, p. 227), "Hitherto I have shown the properties, transmutations, and reductions of all sorts of equations. I have not always joined the demonstrations, because they seemed too easy to need it, and sometimes cannot be laid down without too much tediousness."

In reference to the series of limiting equations marked (I) below, I may here mention a property which I believe has not previously been observed:—

If the middle one of any three of the consecutive functions be squared, the first two terms of the result multiplied by the proper Newtonian factor (as suggested by the degree of the polynomial), will always be the same as the first two terms in the product of the extremes multiplied by the corresponding Newtonian factor.

This general property may be demonstrated as follows:—

Let any consecutive three of the functions

$$f(x), f_1(x), \frac{1}{2}f_2(x), \frac{1}{2 \cdot 3}f_3(x), \text{ \&c. } \dots \dots \dots (I)$$

be represented by

$$Ax^p + A'x^{p-1} + \dots$$

$$\frac{1}{m}pAx^{p-1} + \frac{1}{m}(p-1)A'x^{p-2} + \dots$$

$$\frac{1}{m(m+1)}p(p-1)Ax^{p-2} + \frac{1}{m(m+1)}(p-1)(p-2)A'x^{p-3} + \dots$$

The first two terms of the product of the first and third of these expressions, retaining coefficients only, are

$$\frac{1}{m(m+1)}p(p-1)A^2 + \frac{1}{m(m+1)}\{p(p-1) + (p-1)(p-2)\}AA',$$

or

$$\frac{1}{m(m+1)}p(p-1)A^2 + \frac{2}{m(m+1)}(p-1)^2AA'. \quad (1)$$

And the first two terms of the middle expression squared, coefficients only being retained, are

$$\frac{1}{m^2}p^2A^2 + \frac{2}{m^2}p(p-1)AA'. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Multiply (1) by $\frac{m+1}{m} \cdot \frac{p}{p-1}$, and we get (2); that is, multiply the former expression by $(m+1)p$, and the latter by $m(p-1)$, and the results are the same. And these multipliers are Newton's factors, as is easily seen by making m equal to 1, 2, 3, &c., in succession, and putting n , $n-1$, $n-2$, &c., in succession, for p .

An interesting deduction from this general property is, that if Newton's condition hold or fail for the first three terms of any equation, it will in like manner hold or fail for the first three terms of every transformed equation which can result from increasing or diminishing the roots by any quantity whatever. For, the proposed equation being

$$A_n x^n + A_{n-1} x^{n-1} + A_{n-2} x^{n-2} + \dots + A_0 = 0,$$

we know that the first three coefficients of the transformed equation will be the last three of the derived functions (I) when r (the transforming factor) is substituted for x , and therefore that they will be of the forms

$$A_n, \quad ar + A_{n-1}, \quad br^2 + cr + A_{n-2}.$$

But, by the above principle, if the condition hold or fail for the three original coefficients, A_n , A_{n-1} , A_{n-2} , it must equally hold or fail for these, inasmuch as $2n$ times the product of the first and third differs from $n-1$ times the square of the second by exactly the same amount that $2n A_n A_{n-2}$ differs from $(n-1)A_{n-1}^2$. Hence for every transformation these functions of the coefficients always have the same constant difference.

LIV. On *Glacial Submergence*.

By JOHN CARRICK MOORE, *F.G.S.**

THE mathematicians have responded to the geologists, and we are now in a condition to test the value of the theory which attributes the marine shells and drift, at heights 2300 feet above the present sea-level, to a submergence due to the attraction of an ice-cap. I take the formula of Archdeacon Pratt, because his figures are most favourable to the theory, and because his hypothesis of ice thinning out from the pole seems the more reasonable.

* Communicated by the Author.

First, let us suppose that the ice is obtained at the expense of the sea; in other words, suppose that the quantity of ice at the south pole, when the north is under glacial conditions, is the same as at present. The cap extends to lat. 35° , where, by Archdeacon Pratt's law, it will be 3290 feet thick. Calling the earth's surface 1, this area will be $=0.2132$; and if three-fourths of the remainder, or $\cdot 59$, be sea, δ = the depression of the sea, h' = the mean thickness of the ice-cap, then

$$\delta \times 0.59 = h' \times 0.2132 \times 0.92, \text{ or } \delta = \frac{1}{3} h'.$$

The total quantity of ice forming the cap is $=\pi a^2 \times h \times \cos^2 \text{lat.}$, which, taken from lat. 35° to the pole, $=\pi a^2 \times h \times 0.671$, whence I deduce the mean thickness $h' = h \times 0.787$.

If we take $h=7000$ feet, the assumption of Mr. Croll and of Archdeacon Pratt, $h'=5500$ feet, and $\delta=1833$ feet. Archdeacon Pratt has shown that a thickness of 7000 feet at the pole would produce an elevation of the sea equal to 1000 feet at lat. 60° ; and as it appears that the drain on the sea would lower it 1833 feet, the result is a *depression* of 833 feet.

Let us now suppose that all the ice is supplied by the melting of that at present in south latitudes. The quantity required to produce a submergence of 2300 feet is $\frac{7000}{1000} \times 2300$, or 16,100 feet of ice at the pole, which, by Archdeacon Pratt's law, will be thinned out to 5300 feet at lat. 35° .

Sir James Ross sailed for hundreds of miles east and west in latitudes higher than 70° . The ice-barrier in lat. 76° and 78° was under 200 feet. The valleys near Mount Herschel are said to be filled with snow several hundred feet thick. If we allow all the Antarctic regions down to lat. 70° to be covered by a uniform cap of ice 2000 feet thick, I think it will be admitted that this is an exaggerated estimate of the existing ice. A simple calculation will show that such a cap, covering, as it does, only 0.06 of the surface of the globe, would not supply the *twentieth* part of the ice required.

It appears to me, therefore, that this theory, however tempting, must be abandoned.

As the quantity of ice to be supplied by the melting of that at the south pole is so greatly disproportionate to its object, it is unnecessary to discuss what appears to me to have been too lightly assumed—viz. that when one pole is under glacial conditions, the opposite will be entirely free from ice.

LV. *On the Change of Eccentricity of the Earth's Orbit regarded as a Cause of Change of Climate.* By the Rev. SAMUEL HAUGHTON, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the January Number of the Philosophical Magazine an interesting paper is published by Mr. James Croll on the Connexion of the "Glacial Epoch" with the Eccentricity of the Earth's Orbit, founded on Leverrier's *Mémoire sur les variations séculaires des éléments des orbites pour les sept planètes principales, Mercure, Vénus, la Terre, Mars, Jupiter, Saturne et Uranus* (Paris 1843), 8vo.

In this memoir M. Leverrier calculates and gives a graphical representation of the eccentricity of the earth's orbit for 100,000 years before the epoch January 1, 1800; from which it appears that the unit of 10,000 years is sufficiently large to develop the law of change, as the eccentricity in 100,000 years attains two *maxima* and two *minima* ranging from 0.0473 to 0.0040, or nearly in the proportion of 12 to 1. Under these circumstances it seems to me that Mr. Croll's assumption of the unit of 50,000 is not only unnecessary, but that it tends to mask the real law of change of eccentricity.

With regard to another and more important point, Mr. Croll's paper appears open to objection. He fixes the times of *greatest cold* at the times of maximum eccentricity, and infers the existence of two glacial epochs; one 200,000 years before A.D. 1800, when the eccentricity was 0.0569, and the other ranging from 950,000 to 750,000 years before the present epoch, with three maxima of eccentricity, viz. 0.0517, 0.0747, and 0.0575. The following are Mr. Croll's words:—

"We have already stated it as our opinion that the glacial epoch of the geologist was the period beginning about 240,000 years ago and extending down till about 80,000 years ago ('Reader,' October 14, December 2 and 9). The time of the greatest cold would be about 200,000 or 210,000 years ago.

"It will be seen from an inspection of the Table that the next glacial epoch prior to this occurred about 750,000 years ago. At that time the eccentricity was exactly equal to what it was 210,000 years ago. Going back 50,000 years further, we find the eccentricity to be only 0.0132. But 50,000 years still further back, viz. 850,000 years ago, the eccentricity almost reached its superior limit. It is quite possible that this, and not 200,000 years ago, may have been the period of the boulder-clay. Proceeding backwards the eccentricity is again found to diminish,

but at the period 950,000 years ago it reached the high value of 0.0517. Here we have three glacial epochs following each other in close succession—or rather, we should say, one long glacial epoch of about 250,000 years broken up by two mild periods 100,000 years apart.”

The foregoing conclusions are irreconcilable with the opinion commonly held by astronomers, that the quantity of heat received from the sun annually *increases* with the increase of the eccentricity; so that the periods selected as glacial epochs by Mr. Croll are probably nearer the times of maximum heat, so far as this is dependent on the eccentricity of the orbit.

The following demonstration of the law, “*That the quantity of heat received per annum from the sun varies inversely as the minor axis of the orbit,*” may be of use to some of your junior readers:—

By Kepler's law of areas

$$r^2 d\theta = h dt, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\begin{aligned} Q \text{ (quantity of heat)} &\propto \frac{dt}{r^2} = \frac{d\theta}{h} \\ &= \frac{d\theta}{\sqrt{\mu a(1-e^2)}} = \frac{\sqrt{a} d\theta}{b\sqrt{\mu}}. \end{aligned}$$

Therefore in one year

$$Q \propto \frac{2\pi\sqrt{a}}{\sqrt{\mu}b} \propto \frac{1}{b}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If we expand $\frac{1}{b}$ in terms of e , we shall have

$$Q \propto \frac{1}{b} \propto 1 + \frac{1}{2}e^2 + \frac{3}{8}e^4 + \frac{5}{16}e^6 + \&c. \quad . \quad . \quad (3)$$

From this expression, neglecting powers of e higher than its square, we find that the minimum amount of heat is received when the orbit is a circle, and that the excess above this minimum varies as the square of the eccentricity.

If the eccentricity of the earth's orbit have a real influence in producing glacial epochs, it would appear from M. Leverrier's calculations that it should be sought for in the *future*, and not in the past; for at about 24,000 years after A.D. 1800, the eccentricity of the earth's orbit will attain a smaller minimum than usual, and the heat receivable from the sun will be less than at any period within 100,000 years before the epoch.

It may be useful here to reproduce M. Leverrier's Table.

Before A.D. 1800.	100,000	Eccentricity.	0·0473
"	90,000	"	0·0452
"	80,000	"	0·0398
"	70,000	"	0·0316
"	60,000	"	0·0218
"	50,000	"	0·0131
"	40,000	"	0·0109
"	30,000	"	0·0151
"	20,000	"	0·0188
"	10,000	"	0·0187
"	Epoch	"	0·0168
After A.D. 1800.	10,000	"	0·0115
"	20,000	"	0·0047
"	30,000	"	0·0059

In round numbers, during these 130,000 years, the eccentricity varied from 0·05 to 0·005; therefore its square varied from 0·0025 to 0·000025, or from $\frac{1}{400}$ to $\frac{1}{40,000}$.

In equation (3) unity represents the total annual heat received from the sun when the earth's orbit is circular, and $\frac{1}{2}e^2$ represents on the same scale the increment due to eccentricity; hence the total heats per annum received at the epoch 100,000 before A.D. 1800, and receivable at 25,000 after A.D. 1800, are to each other as

$$1 + \frac{1}{800} \text{ to } 1 + \frac{1}{80,000},$$

or as 80,100 to 80,001, showing an amount of change of $\frac{1}{800}$ th between the epochs.

This change of quantity of heat is very considerable, and capable of producing important results; but whether it is capable of producing a so-called "glacial epoch" would be difficult to say.

I am, yours respectfully,

Trinity College, Dublin,
April 16, 1866.

SAMUEL HAUGHTON.

LVI. Note on Dr. Siemens's Paper "*On the Question of the Unit of Electrical Resistance.*" By A. MATTHIESSEN, F.R.S.*

IT is not my intention to open a discussion on this subject, as it has already been fully entered upon in the Reports of the British Association for 1862-64. I will, however, point out

* On applying to the Editor of the Philosophical Magazine to insert this Note in answer to the paper which appeared in Poggendorff's *Annalen*, No. 2, 1866, he informed me that he had received from the author a translation of it for insertion in the present Number, and that therefore the quotations had better be made from the English version. This I have done.

some of the mistakes which Dr. Siemens has made in his paper, probably owing to his not having carefully read the Reports and papers he criticises.

Page 327, Dr. Siemens states, "I am rather surprised that Dr. Matthiessen should have observed in German silver marked changes within short periods of time, as I have always found this alloy remarkably constant." In the Report of 1863 (p. 126) I point out that, although the conducting-power of the German silver experimented with altered, yet this is no proof that all German silver will do so. Again, in Report for 1864 (p. 347), it is remarked, "A somewhat capricious change has been observed in certain annealed German-silver wires, while others have been proved constant [referring to my experiments in Report of the same year, p. 352]. This result has been independently observed by other members of the Committee."

Page 327, Dr. Siemens states, "It is true Dr. Matthiessen brings forward an argument in favour of the electrical permanency of an alloy of silver and gold—in which I can scarcely suppose him to be serious: it is, that we have never found a gold chain to become brittle." In the Report for 1862 (p. 149), the passage referring to this will be found in that part of the Report where we discuss the effects of homogeneity and molecular condition on the gold-silver alloy as a means of reproducing standards, and is as follows:—"It has been argued that the molecular condition of all alloys is liable to undergo a change by age, and that therefore alloys are not fit to be used as standards. Thus it is well known that brass and German silver become brittle and crystalline by age, and that the same may occur with the gold-silver alloy; but on looking at the composition of the alloy, it will be found to have nearly the same as that of the gold chains of commerce. Now we do not know of a single instance where such a chain, even after years of use, becomes brittle or crystalline by age; so that we think it more than possible that the alloy will not change its molecular condition by age."

Page 328, Dr. Siemens states, "On this ground it is certainly significant that the Committee should have made ten normal measures instead of one single one, even supposing them to agree with each other to within 0.03 per cent., as asserted." Dr. Siemens should have added after the words within 0.03 per cent., *at 15°·5, and are equal to one another at some temperature stated on each coil, lying between 14°·5 and 16°·5 C.**

With regard to my assertion †, that no true mercury unit has

* Proceedings of the Royal Society, No. 74, p. 160. The translation of this paper (Poggendorff's *Annalen*, vol. cxxvi. p. 369) is afterwards quoted by Dr. Siemens.

† Phil. Mag. May 1865, p. 364. Poggendorff's *Annalen*, vol. cxxv. p. 91.

been issued, I think this is sufficiently proved by Dr. Siemens's statement (page 331) that the "standards will not represent the unit at the temperature stated, but at $10^{\circ}5$ higher"*. I merely mention this to point out a probable error in this value ($10^{\circ}5$); for to obtain it, Dr. Siemens uses a coefficient for the correction of resistance for temperature of the units which appears very low, namely 0.0272 per cent. for each degree Centigrade—in fact a much lower one than that sent with the units distributed in 1863 or 1864 (the so-called 1864 unit), namely 0.043 per cent. for each degree.

Page 332, Dr. Siemens states, "The proof advanced by Dr. Matthiessen† in support of his assertion that the units I have determined do not represent the same resistance, is based upon the measurement of two of my resistance-coils, of each from 1 to 10,000 units, by Mr. F. Jenkin, who was employed by the jurors of the 1862 Exhibition to report upon the electrical instruments. Mr. Jenkin found a difference of 1.2 per cent. between the values of these two sets of resistances." Dr. Siemens then blames me for using these values, as the coils were only adjusted for practical use, &c.

My reason for using these values in my paper will, I think, be perfectly justified when it is considered that (1) in the Report of 1862 (p. 147), these same values are brought forward when discussing mercury as a means of reproducing standards. This Report had been published for about two years, and during that time no explanation of these differences has been published. (2) In the Jurors' Report, Mr. Jenkin, when speaking of these coils, states, "Each set of coils exhibited by the London and Berlin firms is adjusted with an accuracy of one per thousand, but those exhibited by the two firms differ from one another by more than 1 per cent. Messrs. Siemens suggest that this difference may be due to an alteration in the German-silver wire of which the coils were made On the other hand, Messrs. Siemens inform the writer that the two coils were really made in Berlin about the same time. They suggest that when compared by the writer, they may not have had the same temperature; but precautions were taken to prevent mistakes of this kind, and a difference of temperature of about 45° F. would be required to account for the difference of resistance observed." And (3) that Mr. Jenkin‡ considers that the values given in his Table may possibly be 0.1 per cent. wrong, but does not think any of them 0.25 per cent. wrong.

Page 333, Dr. Siemens states, "Dr. Matthiessen asserts,

* In Poggendorff's *Annalen* (p. 337) this stands "at $10^{\circ}5$ lower."

† The term I used was *issued*.

‡ Phil. Mag. May 1865, p. 366, foot-note.

further, that the set of coils adjusted by this improved system differ 0·5 per cent. from the standards of 1864. He concludes this from measurements of a copper wire which Mr. F. Jenkin, during the Exhibition of 1862, compared with the sets of coils. What temperature the copper wire had in the two measurements, with an interval of some years between them, is not given. A difference of $1\frac{1}{2}$ degree Centigrade would, however, account for the apparent discrepancy."

The passage referring to the above is, I suppose (Phil. Mag. May 1865, p. 365), where I speak of *a coil whose resistance equals a mile of pure copper wire* (the copper wire quoted by Dr. Siemens) *equalling in resistance 13·95 Siemens (London), and 14·12 (Berlin) units.* The next sentence is as follows:—

"Now, when I adjusted this mile coil for Messrs. Elliotts Brothers, I made another coil of the same resistance of different German-silver wire, &c.;" so that the "copper wire" was an adjusted coil made of German-silver wire, and therefore of the same material as the coils with which the comparisons were made. The whole passage is too long to be quoted *in extenso*; but the above will show that Dr. Siemens has somewhat misunderstood it.

With regard to the 0·8 per cent. difference existing between the reproduction of the mercury unit by Dr. Siemens and Mr. Sabine on the one side, and Mr. Hockin and myself on the other, Dr. Siemens states (p. 334), "I do not think that Dr. Matthiessen should have compared the results of his determinations with mine, as he departed in several material points from the method I laid down." He enumerates the following points:—

(1) As to the formula used for the correction for conicalness, he asserts that that used by Mr. Hockin and myself is not so correct as one as that used by himself. On referring the question to a mathematician, he informed me that our formula was quite as correct as that used by Dr. Siemens (both being only approximate formulæ).

(2) Dr. Siemens takes objection to our method of determining the weight of the tube full of mercury. In Dr. Siemens's own weighings (Phil. Mag. January 1861, p. 38) the maximum discrepancy between the weighings of mercury from the same tube is equal to 0·03 per cent., between Mr. Sabine's (Phil. Mag. March 1863, p. 161) 0·03 per cent., and between ours (Report, 1864, p. 364) 0·07 per cent. Had the soft skin of the points of the fingers been pressed into the openings of the tube when taking it out of the trough, as suggested by Dr. Siemens, we should scarcely have obtained such concordant weighings as we did; for with the first tube the maximum difference between

the weighings is 0·011 per cent. ; with the second, 0·007 per cent. ; and with the third, 0·07 per cent.

(3) Dr. Siemens says that we ought to have inverted the tubes in the bridge, and taken the readings in both directions. We did not do this, but we did what amounted to the same ; we took the zero-point of the scale immediately after each series of readings. It must be remembered that our bridge was so arranged that a millimetre movement of block corresponds to a change in the ratio of the resistances of about 0·01 per cent., whereas in Dr. Siemens's bridge it corresponds to 0·4 per cent. ; also that we used as bridge-wire a platinum-iridium alloy (2 millims. diameter), which on heating varies in conducting-power much less than platinum, of which Dr. Siemens's bridge-wire (0·16 millim. diameter) was made.

One point which neither Dr. Siemens nor Mr. Sabine mention in their papers is, whether their weights and metre-scales were normally accurate ; I mean whether they took the precaution to ascertain, as we did with ours, that their gramme and metre were an absolute gramme and metre. I do not for a moment doubt that their grammes, or the divisions of their scales, agreed well with one another ; in fact they must have done so, otherwise the results obtained could not agree so well with each other. Also they do not state whether they used the method of double weighing or not. All our weighings were made in this manner.

I must leave it to others to judge whether the experiments made by Mr. Hockin and myself with mercury have not been carried out with as great a care and with as many precautions to ensure correct results as those by Dr. Siemens or Mr. Sabine ; and in conclusion I would point out that as the metrical system is gaining yearly more and more ground, in science as well as in every-day life, chiefly owing to its forming a coherent system of measurements (length, weight, capacity, surface), so also will doubtless the B. A. unit, forming a part of a coherent system of electrical measurements (resistance, current, quantity, electro-motive force), gradually gain ground and supplant all others.

LVII. *Supplemental Note on the Analogues in Space to the Cartesian Ovals in plano.* By Professor SYLVESTER, LL.D., F.R.S.*

TO complete the theory given in the last Number of the Magazine, concerning the new ovals in space, I ought to notice that the focal cubic there spoken of is the circular cubic of which the four points in the circle, by means of which it is determined, are the foci. This will become evident from com-

* Communicated by the Author.

parison with Dr. Salmon's 'Higher Plane Curves,' p. 175. My focal cubic is the locus of one set of foci of a system of conics whose axes are parallel, which pass therefore through four points lying in a circle. The axis in which the foci are taken, and which is parallel to the real asymptote, in general meets the focal curve in two points. Whenever these points come together, this parallel to the asymptote becomes a tangent; and the foci do come together for the circle itself and for the three pairs of lines which can be drawn through the four points in question. Hence the focal cubic not only passes through the centre of the circle and through the intersections of the three pairs of lines just spoken of, but at each of these four points is parallel to the real asymptote, *i. e.* to the line bisecting one of the angles in which the diagonals cross. It has also two circular points at infinity. All these conditions are fulfilled by one of Dr. Salmon's pair of circular cubics, of which the four points in question are the foci. These curves are therefore identical; or, to express the same idea more fully, the *two* conjugate circular cubics, of which four points in a circle are the foci, together constitute the *complete* locus of the foci of the system of conics which can be drawn through those four points*. It is interesting, moreover, to notice that the spherical curve which is the intersection of any two right cones with parallel axes, and which is necessarily contained also in a third right cone fulfilling the same condition, may be regarded as the *inverse* of any plane section of the spindle or *tore* formed by the revolution of a circle about an axis cutting it, in respect to either point of intersection of the spindle with its axis. The spherical curve in question is of course no other than the so-called pair of twisted Cartesian ovals; and its focal curve may be any of Dr. Salmon's circular cubics of the first kind, *i. e.* one whose four real foci lie in a circle. Finally, if a double-curvature (*i. e.* twisted) Cartesian is given, we may define its focal curve very simply as *one of the two circular cubics of which the points in which it is intersected by the plane passing through the axes of its containing right cones are the four real foci*. The Cartesian itself is contained in a sphere, in a paraboloid of revolution, in three right cones with parallel axes†,

* Hence, as shown by Dr. Salmon, the focal cubic consists of an oval and a serpentine branch. The two associated focal cubics, the same eminent author has shown, may be regarded as the locus of the intersections of similar conics having for their respective pairs of foci the two pairs of points which make up the given set of four foci; but their simpler geometrical definition, as the complete locus of the foci of the conics drawn through the four given points, appears to have escaped observation.

† So remarkable is this property of the three cones, that, at the risk of tedious reiteration, I think it desirable to present it under the same vivid form in which it strikes my own mind. If any two indefinite straight lines

and also in three surfaces of revolution produced by the rotation of cardioids (with their triple foci lying respectively at the points of inflexion of the focal curve) about the stationary tangents*.

If the two parabolas drawn through the four given points lying in a circle which serve to determine the focal curve be

$$x^2 + 2ex + 2fy = 0, \quad y^2 + 2gx + 2hy = 0,$$

I find that the equation to the focal curve which is the locus of the foci lying in the y axis of the conics drawn through the four foci is

$$h(x-e)(x^2+y^2) + (fh-ke+kx)^2 - 2(fh-ke+kx)hy - h^2x^2 = 0.$$

When $x^2 + y^2 = 0$, this gives $(fh-ke+kx-hy)^2 = 0$, showing that $x=0, y=0$ is a *focus*, which demonstrates the focal character of each of the four fixed points.

Departing from the theory of the quasi-Cartesian ovals, if in general we take *any* four fixed points lying at the intersections of the two conics,

$$U = ax^2 + by^2 + 2hxy + 2gzx + 2fxy,$$

and

$$V = \alpha x^2 + \beta y^2 + 2\eta xy + 2\gamma zx + 2\phi xy,$$

cross, they may be regarded as representing a couple of right cones generated respectively by the revolution of the lines about the two bisectors of the angle which they form. Imagine now a quadrangle inscribed in a circle; its diagonals and pairs of opposite sides produced indefinitely will represent three couples of right cones. This triad of couples may be resolved into a couple of triads, the cones of each triad having their axes parallel *inter se* and perpendicular to those of the other triad; the three cones of each triad respectively will have a *common* intersection, the two intersections being consociated twisted Cartesians whose focal cubics are respectively the two consociated circular cubics of which the angles of the quadrangle are the common foci. Moreover each such twisted Cartesian is a spherical curve lying in the sphere of which the circle circumscribed about the quadrangle is a great circle. The verification of these laws of intersection might be used to form the subject of a new and instructive *plate* for students of ordinary *descriptive geometry*.

* It is interesting to trace the change of form in the general double-curvature Cartesians in regard to the real and imaginary. For this purpose conceive a sphere penetrated by a conical bodkin of indefinite length; when the point of the bodkin just pricks the sphere externally, the curve consists of a single point; as the bodkin is pushed in, the curve becomes a single oval; when the point of the bodkin again meets the sphere internally, the curve will consist of an oval and a conjugate point; then two ovals are formed; then when the bodkin and sphere touch, of an oval and a conjugate point; then of a single oval; and after the bodkin again touches the sphere, in the other side, of a single point; and finally the curve returns wholly into the *limbo* of the imaginary, whence it originally issued. There is apparently nothing analogous to this in the geometrical genesis of the plane Cartesian ovals.

the foci of the conic $U + \lambda V$ will be given by the equality

$$\begin{vmatrix} a + \lambda\alpha & h + \lambda\eta & g + \lambda\gamma & 1 \\ h + \lambda\eta & b + \lambda\beta & f + \lambda\phi & i \\ g + \lambda\gamma & f + \lambda\phi & 0 & -(x + iy) \\ 1 & i & -(x + iy) & 0 \end{vmatrix} = 0,$$

which, by equating real and imaginary parts, gives two equations between x, y, λ .

By aid of these equations λ may be expressed as a rational integral function of x, y , which we know *a priori* must be of the second degree only, since otherwise, on substituting for its value in either of the equations between x, y, λ , we should obtain an equation above the sixth degree in x, y , contrary to Chasles's theorem (we shall also see that this is the case, without having recourse to this theorem, by the reasoning below).

Let $x^2 + y^2 = 0$, then the above equality becomes

$$(f - ig + (\phi - i\gamma)\lambda)^2 = 0,$$

showing that the origin, *i. e.* any one at will and therefore all of the four fixed points, is a focus. If λ were above the second degree in x, y , the line joining this point with either of the circular points of infinity would be *always* at least a triple tangent to the focal curve; but in the case where the focal curve breaks up into two distinct subloci, we have seen that these tangents to each sublocus are simple, *i. e.* that they are double in regard to the whole locus, wherefore λ must be always a quadratic function only of x, y . Consequently each circular point of infinity must itself be a double point upon the curve.

If now we regard the four given points as syzygetic foci of a curve (a term indispensable to give precision to the theory of foci when interpreted in the Plückerian sense), *i. e.* if we suppose a plane curve defined by the equation

$$\lambda\sqrt{A} + \mu\sqrt{B} + \nu\sqrt{C} + \pi\sqrt{D} = 0,$$

where A, B, C, D are the characteristics of the infinitesimal circles of which the four given points are the centres; and if in the norm of the linear function above written we make $\lambda \pm \mu \pm \nu \pm \pi = 0$, and conjoin with this two other equations between λ, μ, ν, π , which will make the term $(x^2 + y^2)^3(Lx + My)$ in the norm vanish identically, *i. e.* the equations $L = 0, M = 0$, we shall obtain a group* of curves of the sixth degree, each possessing precisely the same geometrical characters as have been proved to be satisfied by the curve of foci of the conics $U + \lambda V$ drawn through the four fixed points, viz. of having the circular

* No two of the group can be identical; for in such case one of the focal distances could be eliminated, and the syzygy be reducible by one term, contrary to hypothesis.

points at infinity for double points, and being doubly touched* by each line joining either of them with any one of the four fixed points; and if we are at liberty to assume (which, however, requires further investigation†) that the curve containing the foci of $U + \lambda V$ must be identical with one of the group, then this curve of foci will be defined by the equation

$$l\sqrt{A} + m\sqrt{B} + n\sqrt{C} + p\sqrt{D} = 0;$$

whence, calling a, b, c, d the four fixed points, and F, G, H the points of intersection of the opposite sides of the quadrangle a, b, c, d , the signs of the square roots below written must be capable of being so assumed that the determinant

$$\begin{vmatrix} (aF)^{\frac{1}{2}} & (bF)^{\frac{1}{2}} & (cF)^{\frac{1}{2}} & (dF)^{\frac{1}{2}} \\ (aG)^{\frac{1}{2}} & (bG)^{\frac{1}{2}} & (cG)^{\frac{1}{2}} & (dG)^{\frac{1}{2}} \\ (aH)^{\frac{1}{2}} & (bH)^{\frac{1}{2}} & (cH)^{\frac{1}{2}} & (dH)^{\frac{1}{2}} \\ 1 & 1 & 1 & 1 \end{vmatrix} \text{ shall be equal to zero,}$$

constituting a remarkable theorem concerning seven points (four quite arbitrary) in a plane. If (as seems probable) the case supposed is what actually obtains, a geometrical rule must exist for determining the proper combination of signs to be employed in the above determinant; and then $l; m; n; p$ will be proportional to the first minors of the three first lines of the matrix above written‡.

The above theory, very hastily sketched out under the pressure of other occupations, will serve at all events to manifest in how very imperfect and incoherent a form the theory of foci at present exists, and may serve to raise the question whether there is not a family of curves distinguishable by the possession of syzygetic foci, and which may be termed syzygetic or norm curves (including as elementary members of the group conics, Cartesian

* In general, if a curve is defined by a homogeneous linear relation between its distances from r , or a non-homogeneous linear relation between its distances from $r-1$ points, it will easily be seen that the line joining each such point with either circular point at infinity will be a tangent touching the curve in 2^{r-2} points.

† To the group of syzygetic curves there are only eight parameters, and these eight tangents (all double) at the four foci are common to each member of the group and to the locus of the foci of $U + \lambda V$. To complete the proof of the supposed identity of the latter with one of the former, it is necessary to show that the number of curves having the eight tangents in question is precisely equal to the number of systems of solutions of the equations

$$l + m + n + p = 0,$$

$$L = 0, \quad M = 0.$$

‡ Calling the determinant D , the equation $D = 0$ serves to fix the allowable combinations of the doubtful signs, just as in Cardan's rule a certain equation, which the product of the two associated cube-roots in the solution is bound to satisfy, fixes their allowable combinations of values.

ovals, circular cubics, &c.), forming a distinct genus, and calling for a special and detailed examination of their focal properties*.

* In an evil hour for the cause of sound nomenclature, and by an over-hasty generalization, a property of foci properly so called was substituted for their true definition as centres of linear relationship. The temptation was great, it must be allowed; for the new definition gave a means of describing each focus *per se* without reference to the associated points: it calls to mind the analogous attempted definition of *man* as a *featherless biped*, and is open to the like kind of objections. The mischief being done, and under cover of the authority of names so great that to root it out seems now hopeless, the best remedy to apply is, as I think, that used in the text, of distinguishing the centres of linear relationship as *syzygetic foci* or *foci proper*. There is room for a grand chapter in the promised and anxiously-expected new edition of Dr. Salmon's 'Higher Plane Curves,' on a systematic and exhaustive development of the laws of foci proper, and the algebraical philosophy, as it may well be termed, of true focal curves, *i. e.* curves the distances of whose points from one or more sets of fixed points are subject to linear relations. Nothing can be more curious than the study of the way in which, starting from a given set of fixed points, other foci (as in the Cartesian ovals) are found capable of replacing one or more of the given ones, constituting the theory of substitution—and then, again, how, as in the conic sections and in circular cubics, besides this faculty of mutual substitutability of foci of the same set, one set may be entirely replaced by one or more other sets, constituting the theory of plurality or distribution. Algebra cannot but gain largely by these ideas of substitution and distribution being fully worked out.

In answer to my objections to the undue extension of the term *focus*, it has been urged that a focus, as originally presenting itself in the theory of conics, is susceptible of two distinct definitions—first as a member of a syzygetic group, and again as a point whose squared distance from any point in its curve is the square of a linear function of the coordinates,—that it is legitimate to generalize the conception from either of these points of view, and that the latter leads to the definition of a focus as a point whose squared distance from any point in its curve, multiplied by a quantic, gives rise to a second quantic containing a squared linear function as a factor. But I answer to this, that the generalization is carried too far and too fast, two steps in enlargement of the original condition being taken at once to arrive at it; that the first step should be to define a focus as a point such that the squared distance in question, multiplied by a quantic, viewed as a function of the coordinates, shall be a perfect square; and that when this first step is taken, the foci so obtained are foci of a peculiar kind, and probably retain their quality as *foci proper*, or centres of linear relationship. At all events they possess the property of giving, by their junctions with the circular points at infinity, multiple tangents to the curve, according to the law stated in a previous foot-note concerning such foci.

If the word focus is retained to signify the proper or syzygetic species, some slight modification of the word may be used to denote the genus, *viz.* foci which satisfy the larger definition of being points of intersection of the simple tangents to the circular points at infinity. I thought of the word *focal* for the purpose; but this is objectionable, for the reason that it would probably be found advisable to retain that word to denote the class of *curves* which possess foci proper. On the whole, the word *subfocus* seems to me best to meet the exigency of the case, and possesses the recommendation of being capable (with dialectic variations) of passing current in each of the five accepted tongues—Latin, German, French, English, and Italian, which happily at the present day may be regarded as the common property and inheritance of mathematical Europe.

LVIII. *On Calorescence.*

By Professor J. TYNDALL, LL.D. Camb., F.R.S. &c.*

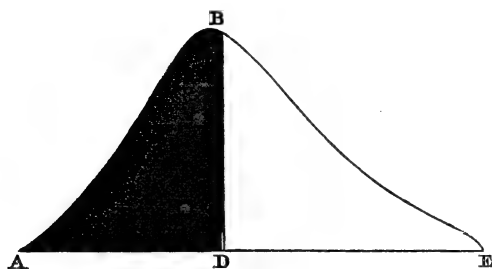
Forsitan et roseâ sol altè lampade lucens
 Possideat multum cæcis fervoribus ignem
 Circum se, nullo qui sit fulgore notatus,
 Æstiferum ut tantum radorum exaugeat ictum.

LUCRET. v. 610†.

§ 1. **I**N the year 1800, and in the same volume of the Philosophical Transactions that contains Volta's celebrated letter to Sir Joseph Banks on the Electricity of Contact ‡, Sir William Herschel published his discovery of the invisible rays of the sun. Causing thermometers to pass through the various colours of the solar spectrum, he determined their heating-power, and found that this power, so far from ending at the red extremity of the spectrum, rose to a maximum at some distance beyond the red. The experiment proved that, besides its luminous rays, the sun emitted others of low refrangibility, which possessed great calorific power, but were incompetent to excite vision.

Drawing a datum-line to represent the length of the spectrum, and erecting at various points of this line perpendiculars to represent the calorific intensity existing at those points, on uniting the ends of the perpendiculars Sir William Herschel obtained the subjoined curve (fig. 1), which shows the distribution of

Fig. 1.



Spectrum of Sun (Herschel) reduced.

heat in the solar spectrum, according to his observations. The space A B D represents the invisible, and B D E the visible radiation of the sun. With the more perfect apparatus subsequently devised, Professor Müller of Freiburg examined the distribution of heat in the spectrum §, and the results of his observations are

* From the Philosophical Transactions for 1866, Part I. Communicated by the Author.

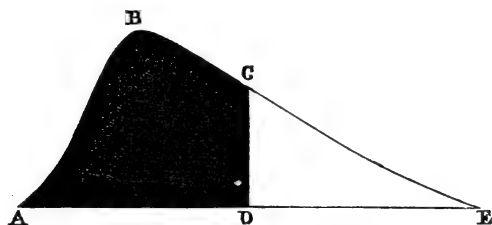
† I am indebted to my excellent friend Sir Edmund Head for this extract, which reads like divination.

‡ Vol. lxx.

§ Philosophical Magazine, S. 4. vol. xvii. p. 242.

rendered graphically in fig. 2. Here the area A B C D represents the invisible, while C D E represents the visible radiation.

Fig. 2.



Spectrum of Sun (Müller).

With regard to terrestrial sources of heat, it may be stated that all such sources hitherto examined emit those obscure rays. Melloni found that 90 per cent. of the emission from an oil-flame, 98 per cent. of the emission from incandescent platinum, and 99 per cent. of the emission from an alcohol-flame consists of obscure rays*. The visible radiation from a hydrogen-flame is, according to my own experiments, too small to admit of measurement. With regard to solid bodies, it may be stated generally that, when they are raised from a state of obscurity to vivid incandescence, the invisible rays emitted in the first instance continue to be emitted with augmented power when the body glows. For example, with a current of feeble power the carbons of the electric lamp may be warmed and caused to emit invisible rays. But the intensity of these same rays may be augmented a thousandfold by raising the carbons to the temperature necessary for the electric light. Here, in fact, the luminous and non-luminous emission augment together, the maximum of brightness of the visible rays occurring simultaneously with the maximum calorific power of the invisible ones†.

At frequent intervals during the past ten or twelve years I have had occasion to experiment on the invisible rays of the electric light, and have finally made them the subject of special investigation. The present paper contains a brief account of the inquiry. I endeavour, in the first place, to compare the luminous with the non-luminous radiation of the electric light, and to determine their relative energy; I point out a method of detaching the luminous from the non-luminous rays; and afterwards describe various experiments illustrative of the calorific power of the invisible rays, and of the transmutations of which they are capable.

§ 2. The instrument employed by Professor Müller in the investigation above alluded to, was a form of the thermo-electric

* *La Thermochrose*, p. 304.

† On this point see the Rede Lecture for 1865, p. 33 (Longmans).

pile devised by Melloni for the examination of this and kindred questions. Through the kindness of my friend Mr. Gassiot, a very beautiful instrument of this kind, constructed by Ruhmkorff, has remained in my possession for several years, and been frequently employed in my researches. It consists of a double metallic screen, with a rectangular aperture in the centre, a single row of thermo-electric elements 1·2 inch in length being fixed to the screen behind the aperture. Connected with the latter are two moveable side pieces, which can be caused to approach or recede so as to vary the width of the exposed face of the pile from zero to $\frac{1}{10}$ th of an inch. The instrument is mounted on a slider, which, by turning a handle, is moved along a slot on a massive metal stand. A spectrum of a width equal to the length of the thermo-electric pile being cast at the proper elevation on the screen, by turning the handle of the slider the vertical face of the pile can be caused to traverse the colours, and also the spaces right and left of them.

To produce a steady spectrum of the electric light, I employed the regulator devised by M. Foucault and constructed by Duboseq, the constancy of which is admirable. A complete rock-salt train was constructed for me by Mr. Becker, and arranged in the following manner:—In the front orifice of the camera which surrounds the electric lamp was placed a lens of transparent rock-salt, intended to reduce to parallelism the divergent rays proceeding from the carbon-points. The parallel beam was permitted to pass through a narrow slit, in front of which was placed another rock-salt lens, the position of this lens being so arranged that a sharply-defined image* of the slit was obtained at a distance beyond it equal to that at which the spectrum was to be formed. Immediately behind this lens was placed a pure rock-salt prism (sometimes two of them). The beam was thus decomposed, a brilliant horizontal spectrum being cast upon the screen which bore the thermo-electric pile. By turning the handle already referred to, the face of the pile could be caused to traverse the spectrum, an extremely narrow band of light or radiant heat falling upon it at each point of its march†. A sensitive galvanometer was connected with the pile, and from its deflection the heating-power of every part of the spectrum, visible and invisible, was determined.

Two modes of moving the instrument were practised. In the first the face of the pile was brought up to the violet end of the spectrum, where the heat was insensible, and then moved through the colours to the red, then past the red up to the position of maximum heat, and afterwards beyond this position until the heat of the invisible spectrum gradually faded away. The fol-

* The width of the image was about 0·1 of an inch.

† The width of the linear pile was 0·03 of an inch.

Following Table contains a series of measurements executed in this manner. The motion of the pile is measured by turns of its handle, every turn corresponding to the shifting of the face of the instrument through a space of one millimetre, or $\frac{1}{25}$ th of an inch. At the beginning, where the increment of heat was slow and gradual, the readings were taken at every two turns of the handle; on quitting the red, where the heat suddenly increases, the intervals were only half a turn, while near the maximum, where the changes were most sudden, the intervals were reduced to a quarter of a turn, which corresponded to a translation of the pile through $\frac{1}{100}$ th of an inch. Intervals of one and of two turns were afterwards resumed until the heating-power ceased to be distinct. At every halting-place the deflection of the needle was noted, the value of the deflection, referred to the first degree as unit, being placed in the first column of figures in the Table. It was found convenient to call the maximum effect in each series of experiments 100; the second column of figures, obtained by multiplying the first by the constant factor 1.37, expresses the heat of all the parts of the spectrum with reference to this maximum.

Table I.—Distribution of Heat in Spectrum of Electric Light.

Movement of pile.	Value of deflection.	Calorific intensity, in 100ths of the maximum.
Before starting (pile in the blue)	0.0	0.0
Two turns forward (green entered)	1.5	2.0
„	3.5	4.8
„	5.5	7.5
„ (red entered)	15.5	21.0
„ (extreme red)	32.6	44.6
Half turn forward	44.0	60.0
„	54.0	74
„	62.0	85
„	70.0	95.8
„	72.5	99
Quarter turn forward, <i>maximum</i> .	73.0	100.0
„	70.8	97.0
Half turn forward	57.0	78.0
„	45.5	62.0
„	32.6	44.5
„	26.0	35.6
Two turns forward	10.5	14.4
„	6.5	9
„	5.0	6.8
„	3.5	5
„	2.5	3.4
„	1.7	2.3
„	1.3	1.8

Here, as before stated, we begin in the blue, and pass first through the visible spectrum. Quitting this at the place marked "(extreme red)," we enter the invisible calorific spectrum and reach the position of maximum heat, from which, onwards, the thermal power falls till it practically disappears.

In other observations the pile was first brought up to the position of maximum heat, and moved thence to the extremity of the spectrum in one direction. It was then brought back to the maximum, and moved to the extremity in the other direction. There was generally a small difference between the two maxima, arising, no doubt, from some slight alteration of the electric light during the period which intervened between the two observations. The following Table contains the record of a series of such measurements. As in the last case, the motion of the pile is measured by turns of the handle, and the values of the deflections are given with reference to a maximum of 100.

TABLE II.—Distribution of Heat in Spectrum of Electric Light.

Movemnt of pile.		Calorific intensity in 100ths of the maximum.
Maximum	100
One turn <i>towards</i> visible spectrum	94.4
" "	65.5
" "	42.6
" "	(extreme red).	28.3
" "	20.0
" "	14.8
" "	11.1
Two turns in the same direction (green entered).		7.4
" "	4.6
" "	2.0
" "	(pile in blue).	0.9
Pile brought back to maximum.		
Maximum	100
One turn <i>from</i> visible spectrum	67.1
" "	41.0
" "	23.0
" "	13.0
" "	9.4
Two turns	5.0
"	3.4
"	0.0

More than a dozen series of such measurements were executed, each series giving its own curve. On superposing the different curves, however, a very close agreement was found to exist be-

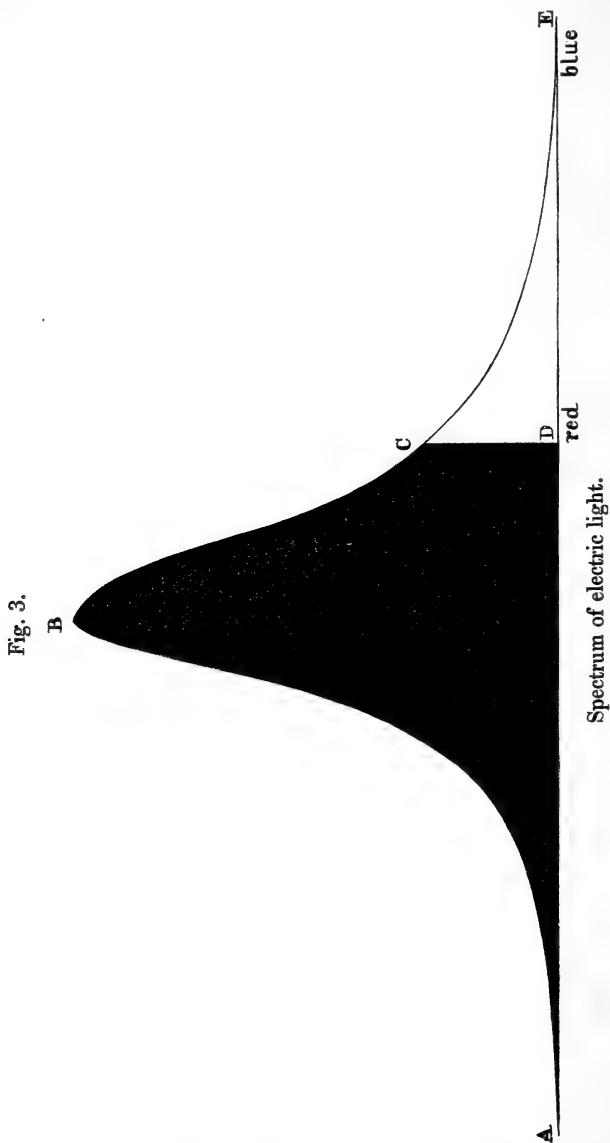
tween them. The annexed curve (fig. 3), which is the mean of several, expresses, with a close approximation to accuracy, the distribution of heat in the spectrum of the electric light from fifty cells of Grove. The space A B C D represents the invisible, while C D E represents the visible radiation. We here see the gradual augmentation of thermal power, from the blue end of the spectrum to the red. But in the region of dark rays beyond the red the curve shoots suddenly upwards in a steep and massive peak, which quite dwarfs by its magnitude the portion of the diagram representing the visible radiation*.

The sun's rays before reaching the earth have to pass through our atmosphere, where they encounter the atmospheric aqueous vapour, which exercises a powerful absorption on the invisible calorific rays. From this, apart from other considerations, it would follow that the ratio of the invisible to the visible radiation in the case of the sun must be less than in the case of the electric light. Experiment, we see, justifies this conclusion; for whereas fig. 2 shows the invisible radiation of the sun to be about twice the visible, fig. 3 shows the invisible radiation of the electric light to be nearly eight times the visible. If we cause the beam from the electric lamp to pass through a layer of water of suitable thickness, we place its radiation in approximately the same condition as that of the sun; and on decomposing the beam after it has been thus sifted, we obtain a dis-

* How are we to picture the vibrating atoms which produce the different wave-lengths of the spectrum? Does the infinity of the latter, between the extreme ends of the spectrum, answer to an infinity of atoms each oscillating at a single rate? or are we not to figure the atoms as virtually capable of oscillating at different rates at the same time? When a sound and its octave are propagated through the same mass of air, the resultant motion of the air is the algebraic sum of the two separate motions impressed upon it. The ear decomposes this motion into its two components (Helmholtz, *Ton-Empfindungen*, p. 54); still we cannot here figure certain particles of the air occupied in the propagation of the one sound, and certain other particles in the propagation of the other. May not what is true of the air be true of the æther? and may not, further, a single atom, controlled and jostled as it is in solid bodies by its neighbours, be able to impress upon the æther a motion equivalent to the sum of the motions of several atoms each oscillating at one rate?

It is perhaps worthy of remark, that there appears to be a definite rate of vibration for all solid bodies having the same temperature, at which the *vis viva* of their atoms is a maximum. If, instead of the electric light, we examine the lime-light, or a platinum wire raised to incandescence by an electric current, we find the apex of the curve of distribution (B, fig. 3) corresponding throughout to very nearly, if not exactly, the same refrangibility. There seems, therefore, to exist one special rate at which the atoms of heated solids oscillate with greater energy than at any other rate—a non-visual period, which lies about as far from the extreme red of the spectrum on the invisible side as the commencement of the green on the visible one.

tribution of heat closely resembling that observed in the solar spectrum.



The curve representing the distribution of heat in the electric spectrum falls most steeply on that side of the maximum which

is most distant from the red. On both sides, however, we have a *continuous* falling off. I have had numerous experiments made to ascertain whether there is any interruption of continuity in the calorific spectrum; but all the measurements hitherto executed with artificial sources reveal a gradual and continuous augmentation of heat from the point where it first becomes sensible up to the maximum. Sir John Herschel has shown that this is not the case with the radiation from the sun when analyzed by a flint-glass prism. Permitting the solar spectrum to fall upon a sheet of blackened paper, over which had been spread a wash of alcohol, this eminent philosopher determined by its drying-power the heating-power of the spectrum. He found that the wet surface dried in a series of spots representing thermal maxima separated from each other by spaces of comparatively feeble calorific intensity. No such maxima and minima were observed in the spectrum of the electric light, nor in the spectrum of a platinum wire raised to a white heat by a voltaic current. Prisms and lenses of rock-salt, of crown glass, and of flint glass were employed in these cases. In subsequent experiments the beam intended for analysis was caused to pass through layers of water and other liquids of various thicknesses. Gases and vapours of various kinds were also introduced into the path of the beam. In all cases there was a general lowering of the calorific power, but the descent of the curve on both sides of the maximum was unbroken*.

§ 3. The rays from an obscure source cannot compete in point of intensity with the obscure rays of a luminous source. No body heated under incandescence could emit rays of an intensity comparable to those of the maximum region of the electric spectrum. If, therefore, we wish to produce intense calorific effects by invisible rays, we must choose those emitted by an intensely luminous source. The question then arises, how are the invisible calorific rays to be isolated from the visible ones. The interposition of an opaque screen suffices to cut off the visible spectrum of the electric light, and leaves us the invisible calorific rays to operate upon at our pleasure. Sir William Herschel experimented thus when he sought, by concentrating them, to render the invisible rays of the sun visible. But to form a spectrum in which the invisible rays shall be completely separated from the visible ones, a narrow slit or a small aperture is necessary; and this circumstance renders the amount of heat separable by prismatic analysis very limited. If we wish to ascertain what the intensely concentrated invisible rays can accomplish, we must devise some other mode of detaching them from their visible

* At a future day I hope to subject this question to a more severe examination.

companions. We must, in fact, discover a substance which shall filter the composite radiation of a luminous source by stopping the visible rays and allowing the invisible ones free transmission.

Could we obtain a *black* elementary body thoroughly homogeneous, and with all its parts in perfect optical contact, experiments already published would lead me to expect that such a body would form an effectual filter for the radiation of the sun or of the electric light. While cutting off the visible radiation, the black element would, I imagine, allow the invisible to pass. Carbon in the state of soot is black, but its parts are not optically continuous. In black glass the continuity is far more perfect, and hence the result established by Melloni, that black glass possesses a considerable power of transmission. Gold in ruby glass, or in the state of jelly prepared by Mr. Faraday, is exceedingly transparent to the invisible calorific rays, but it is not black enough to quench entirely the visible ones. The densely brown liquid bromine is better suited to our purpose; for, in thicknesses sufficient to quench the light of our brightest flames, this element displays extraordinary diathermancy. Iodine cannot be applied in the solid condition, but it dissolves freely in various liquids, the solution in some cases being intensely dark. Here, however, the action of the element may be masked by that of its solvent. Iodine, for example, dissolves freely in alcohol; but alcohol is so destructive of the extra-red rays, that it would be entirely unfit for experiments the object of which is to retain these rays while quenching the visible ones. The same remark applies in a greater or less degree to many other solvents of iodine.

The deportment of bisulphide of carbon, both as a vapour and a liquid, suggests the thought that it would form a most suitable solvent. It is extremely diathermic, and there is hardly another substance able to hold so large a quantity of iodine in solution. Experiments already recorded prove that, of the rays emitted by a red-hot platinum spiral, 94·5 per cent. is transmitted by a layer of the liquid 0·02 of an inch in thickness, the transmission through layers 0·07 and 0·27 of an inch thick being 87·5 and 82·5 respectively*. The following experiment with a layer of far greater thickness exhibits the deportment of the transparent bisulphide towards the more intense radiation of the electric light. A cylindrical cell, 2 inches in length and 2·8 inches in diameter, with its ends stopped by plates of perfectly transparent rock-salt, was placed empty in front of an electric lamp; the radiation from the lamp, after having crossed

* Philosophical Transactions, vol. cliv. p. 333; Philosophical Magazine, S. 4. vol. xxviii. p. 446.

the cell, fell upon a thermo-electric pile, and produced a deflection of
73°.

Leaving the cell undisturbed, the transparent bisulphide of carbon was poured into it: the deflection fell to
72°.

A repetition of the experiment gave the following results:—

	Deflection.
Through empty cell	74
Through bisulphide	73

Taking the values of these deflections from a Table of calibration and calculating the transmission, that through the empty cell being 100, we obtain the following results:—

	Transmission.
For the first experiment . . .	94·9
For the second experiment . . .	94·6
Mean	94·8

Hence the introduction of the bisulphide lowers the transmission only from 100 to 94·8*.

A *perfect* solvent of the iodine would be perfectly neutral to the total radiation; and the bisulphide of carbon is shown by the foregoing experiment to approach tolerably near perfection. We have in it a body capable of transmitting with little loss the total radiation of the electric light. Our object is now to filter this total, by the introduction into the bisulphide of a substance competent to quench the visible and transmit the invisible rays. Iodine does this with marvellous sharpness. In a short paper "On Luminous and Obscure Radiation," published in the *Philosophical Magazine* for November 1864, the diathermancy of this substance is illustrated by the following Table:—

TABLE III.—Radiation through dissolved Iodine.

Source.	Transmission.
Dark spiral of platinum wire	100
Lampblack at 212° Fahr.	100
Red-hot platinum spiral	100
Hydrogen-flame	100
Oil-flame	97
Gas-flame	96
White-hot spiral	95·4
Electric light, battery of 50 cells . . .	90

These experiments were made in the following way:—A rock-salt cell was first filled with the transparent bisulphide, and the

* The partial destruction of the reflexion from the sides of the cell by the introduction of the bisulphide is not here taken into account.

quantity of heat transmitted by the pure liquid to the pile was determined. The same cell was afterwards filled with the opaque solution, the transmission through which was also determined. Calling the transmission through the transparent liquid 100, the foregoing Table gives the transmission through the opaque. The results, it is plain, refer solely to the iodine dissolved in the bisulphide,—the transmission 100, for example, indicating, not that the solution itself, but that the body dissolved is, within the limits of error, perfectly diathermic to the radiation from the first four sources.

The layer of liquid employed in these last experiments was not sufficiently thick to quench utterly the luminous radiation from the electric lamp. A cell was therefore constructed whose parallel faces were 2·3 inches apart, and which, when filled with the solution of iodine, allowed no trace of the most highly concentrated luminous beam to pass through it. Five pairs of experiments executed with this cell yielded the following results:—

Radiation from Electric Light; battery 40 cells.

	Deflection.
{ Through transparent bisulphide	47·0; 46·0
{ Through opaque solution	42·3; 43·5
{ Through transparent bisulphide	44·0; 43·7
{ Through opaque solution	41·2; 40·0
Through transparent bisulphide	42·0; 43·0

Calling the transmission through the transparent liquid 100, and taking the mean of all these determinations, the transmission through the opaque solution is found by calculation to be 86·8. An absorption of 13·2 per cent. is therefore to be set down to the iodine. This was the result with a battery of forty cells; subsequent experiments with a battery of fifty cells made the transmission 89, and the absorption 11.

Considering the transparency of the iodine for heat emitted by all sources heated up to incandescence, as exhibited in Table III., it may be inferred that the above absorption of 11 per cent. represents the calorific intensity of the *luminous rays* alone. By the method of filtering, therefore, we make the invisible radiation of the electric light eight times the visible. Computing, by means of a proper scale, the area of the spaces A B C D, C D E (fig. 3), the former, which represents the invisible emission, is found to be 7·7 times the latter. Prismatic analysis, therefore, and the method of filtering yield almost exactly the same result.

[To be continued.]

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 317.]

February 1, 1866.—Lieut-General Sabine, President, in the Chair.

THE following communication was read:—

“On the Forms of Graphitoidal Silicon and Graphitoidal Boron.”
By W. H. Miller, M.A., For. Sec. R.S., and Professor of Mineralogy
in the University of Cambridge.

Graphitoidal Silicon.

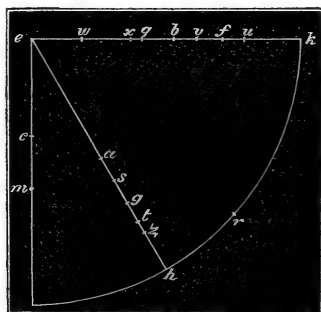
It has been so confidently assumed that graphitoidal silicon is an allotropic condition of silicon crystallized in octahedrons, that on ascertaining by measurement of angles that some graphitoidal silicon given me by Dr. Matthiessen was in simple and twin octahedrons, I at once concluded that the substance had been wrongly named. Later, however, I obtained from Dr. Percy a supply of graphitoidal silicon of unquestionable authenticity. Its lustre was that of the crystals I had previously examined. It occurred in small scales, having for the most part the appearance of crystals of the oblique system. On measurement, however, they proved to be octahedrons in which two parallel faces were much larger than any of the other faces, and two other parallel faces were either too small to be observed or were altogether wanting. One of the scales had all the faces of a twin octahedron. It appears, then, that there is no reason, founded on a difference of form, for separating graphitoidal from octahedral silicon, the sole distinction being that the crystals of the latter are more perfect than those of the former.

Graphitoidal Boron.

The forms of boron have been described by the Commendatore Quintino Sella in two papers read before the Royal Academy of Turin on the 4th of January and the 14th of June, 1857, and by the Baron Sartorius v. Waltershausen in a paper presented to the Royal Society of Göttingen on the 1st of August of the same year. They found independently that the adamantine boron of Wöhler and Deville, containing a variable and not inconsiderable amount of aluminium and carbon, considered by Sella as possibly a definite compound of boron with aluminium and carbon with a mechanical mixture of pure boron, crystallizes in forms belonging to the pyramidal system.

Boron containing 2·4 per cent. of carbon, the boro semplice of Sella, is described by him as occurring in crystals, the faces of which are not so perfect as to admit of a very accurate determination of the angles they make with one another. The angles approximate to some of the angles of crystals of the cubic system, but the aspect of the crystals, which are usually twins, leads to the supposi-

tion that they belong to the oblique system, and that the angle between the oblique axes differs but little from 90° .



The forms observed by Sella, considered as belonging to the oblique system, are:—

$k\ 1\ 0\ 0$, $e\ 0\ 0\ 1$, $c\ 0\ 1\ 3$, $m\ 0\ 2\ 3$, $b\ 1\ 0\ 1$, $n\ 5\ 0\ 4$, $p\ 5\ 0\ 8$, $q\ 2\ 0\ 3$,
 $f\ 2\ 0\ 1$, $h\ 1\ 1\ 0$, $r\ 2\ 1\ 0$, $g\ 1\ 1\ 1$, $a\ 1\ 1\ 2$, $d\ 2\ 1\ 1$, $l\ 2\ 1\ 2$.

Of these, I have since reobserved all, with the exception of a , d , l , and perhaps p , the corresponding reflexion being too faint to enable me to affirm the existence of that face in the crystals I examined. I have also observed the following forms in which the distribution of the faces is in most cases, probably in all, the same as in the prismatic system, or as if the oblique form $h\ k\ l$ were always accompanied by the oblique form $\bar{h}\ \bar{k}\ \bar{l}$:

$u\ 3\ 0\ 1$, $w\ 1\ 0\ 4$, $v\ 4\ 0\ 3$, $x\ 3\ 0\ 5$, $s\ 2\ 2\ 3$, $t\ 3\ 3\ 2$, $z\ 2\ 2\ 1$.

On the same supposition regarding the distribution of the faces, the annexed figure represents an octant of the sphere of projection, the poles of some of the faces not wanted for comparison with those of graphitoidal boron being omitted. The principal angles taken or computed from the angles provisionally adopted by Sella, are:—

ec	$39^\circ\ 14'$	ek	$90^\circ\ 0'$
em	$58\ 31$	km	$90\ 0$
ew	$19\ 28$	kh	$60\ 0$
ex	$40\ 19$	ea	$54\ 44$
eq	$43\ 21$	es	$62\ 4$
eb	$54\ 44$	eg	$70\ 32$
ev	$62\ 4$	et	$76\ 44$
ef	$70\ 32$	ez	$79\ 59$
eu	$76\ 44$	eh	$90\ 0$

Besides the two forms already mentioned, Wöhler and Deville obtained boron in extremely thin scales, which were supposed to be a different modification of boron, and was accordingly called graphitoid-

dal. Sella, however, relying apparently upon the evidence afforded by the lustre and colour of the scales, for he was unable to obtain any measurements, expresses his conviction that they are not different from pure boron. Some scales of this substance, for which, as well as a supply of crystals of pure boron, I am indebted to Dr. Matthiessen, have faces on their edges, but so narrow that the reflected image of the bright signal is diffracted into a line of considerable length, and therefore difficult to bisect. For this reason it is not possible to determine the positions of the faces with accuracy.

One of them, about 2 millims. wide and 0.014 millim. thick, of the shape of half a hexagon divided by a line at right angles to two opposite sides, exhibited faces agreeing in position very fairly, considering the unavoidable errors of observation, with two of the faces *k*, two of the faces *e*, *c*, *m*, three of the faces *b*, two of the faces *x*, *q*, three of the faces *h*, and four of the faces *a*. Another, smaller and thinner, of the shape of a hexagon, had faces coinciding with two of the faces *k*, two of the faces *e*, *c*, *m*, *f*, *v*, and four of the faces *h*. The agreement in position of so many of the faces with those of pure boron appears to leave but little doubt of the identity of the forms of the two substances.

GEOLOGICAL SOCIETY.

[Continued from p. 319.]

February 21, 1866.—Warrington W. Smyth, Esq., President,
in the Chair.

The following communications were read:—

1. "On the Tertiary Mollusca of Jamaica." By R. J. Lechmere Guppy, Esq.

In 1862 Mr. Lucas Barrett deposited in the British Museum a collection of Miocene fossils from Jamaica; and the author having described the nature of the beds whence the fossils were obtained, remarked on the extended development of the Miocene formation in the Caribbean area. From his examination of the fossils, he was able to confirm many of the conclusions arrived at by Mr. Carrick Moore from his investigation of the San-Domingo fossils, and by Dr. Duncan's and Prof. Rupert Jones's investigations of the Corals and Foraminifera of the West-Indian Miocene deposits.

The author considered that the Middle Tertiary beds of San Domingo, Cuba, Cumana, and the Caroni series in Trinidad, together with the Miocene deposits of Jamaica, represent the upper or later part of the West-Indian Miocene; while the chert formation of Antigua, the Anguilla beds, and the beds exposed at San Fernando in Trinidad belong to the lower and older part of the same formation.

Reference was then made to the distinguishing features and characteristic fossils of the beds exposed in the several localities named; and in endeavouring to correlate the beds in the different islands,

the terms Upper and Lower Miocene were used merely as marking what seems to be their relative antiquity. Out of sixty-one species enumerated in this communication, thirty-four have been found to be common to Jamaica and San Domingo, and fourteen to Jamaica and Cumana. Thirteen species are ascertained to be still living, some of which have been found in the Miocene of Europe and other localities. Several of the extinct species exhibit strong eastern affinities; but there is also a resemblance between a part of the fauna and that now existing in the West Indies; and a certain number of the species are allied to European early and middle Tertiary forms. The fauna as a whole is more nearly related to that of Bordeaux, Dax, and Malta than to that of the American Miocene deposits.

2. "On Tertiary Echinoderms from the West Indies." By R. J. Lechmere Guppy, Esq.

The Corals, Shells, and Foraminifera of the West-Indian Miocene having been more or less completely described, the author now brought under notice the Echinoderms belonging to the same fauna, which have been found in Anguilla and Trinidad associated with shells determined to be of Miocene age. The species sufficiently well preserved for determination are nine in number, of which two are found in the Maltese beds; three others, which are new, are closely allied to species found in the same locality. Three out of the nine are still living in the West-Indian seas, but these are rare in the fossil state.

3. "On Tertiary Brachiopoda from Trinidad." By R. J. Lechmere Guppy, Esq.

The beds whence these Brachiopoda were derived have already been mentioned in the previous papers; their organic remains have led to the belief that they belong to a lower horizon in the Miocene series than the beds of Jamaica, Cumana, and San Domingo. The Brachiopoda, which consist of three species of *Terebratula*, can hardly be considered to throw much new light upon the question, as they seem to be suggestive of Cretaceous affinities. As it had been suggested that the fossils in question might be derived from older beds, the reasons which have led the author to an opposite opinion were stated; and it was remarked, in conclusion, that they do not resemble those of Malta.

4. "On the affinities of *Platysomus* and allied genera." By John Young, M.D., F.G.S.

The author described in detail the anatomy of *Platysomus parvulus*, Ag., and two new genera, *Amphicentrum* and *Mesolepis*, all from the North Staffordshire Coal-field, and, after discussing their relations to other ganoids and to the *Teleostei*, proposed their inclusion, with the Pycnodonts and *Eurynotus*, in a distinct suborder of Ganoids.

Suborders I. *Amiadae*, II. *Lepidosteidae*, III. *Crossopterygidae*, IV. *Chondrosteidae*, V. *Acanthodidae*, have already been described

by Prof. Huxley; and the author now gave diagnoses of Suborder VI. *Lepidopleuridæ* (not equivalent to *Pleurolepidæ* of Quenstedt), including the following families:—

1. *Platysomidæ*. Teeth uniserial, conical, sharp; palate edentulous. *Platysomus*, Ag., partim.
2. *Amphicentridæ*. Dorsal and ventral margins sharply angulated; teeth in the form of tuberculated plates on maxillary, mandibular, and palato-vomerine bones; præmaxilla and “præmandibula” edentulous. *Amphicentrum*, n. g.
3. *Euryssomidæ*. Teeth in the form of blunted cones, on a peduncle with a constricted neck. *Euryssomus*=*Platysomus*, Ag., partim.
4. *Mesolepidæ*. Teeth similar to those of *Euryssomus*. *Mesolepis*, n. g., *Eurynotus*, Ag.
5. *Pycnodontidæ*. Teeth oval or hemispherical, or, if elongate, blunted cones. Pycnodonts of authors (except the Labroid forms of Cocchi).

Platysomus ranges from the Carboniferous to the Permian, one species, *P. striatus*, being common to both, as well as to England and Germany. *Euryssomus* is Permian only; the true Pycnodonts exclusively Mesozoic. The remaining families are Carboniferous, while the first three disprove the generalization as to the non-existence of apodal fish before the Chalk.

5. “Note on the Scales of *Rhizodus*, Owen.” By John Young, M.D., F.G.S.

Attention was drawn to the fact that, on a slab in the collection of the Royal Society at Edinburgh, the characteristic *Rhizodus* teeth occur along with thick bony scales whose exposed area is ornamented with coarse tubercles, usually irregularly disposed, while the overlapped anterior area is concentrically striated. These characters confirm the generic distinctness of *Rhizodus* from *Holoptychius*, whose smooth anterior and rugose free surfaces contrast with those described.

LX. Intelligence and Miscellaneous Articles.

ON A GAS-BURNER FOR SOUNDING LARGE TUBES.

BY E. REUSCH.

THE gas-burner which I have used for the last two years in producing from metal pipes, of 2 to 3 metres in length and 7 to 8 centims. in width, beautiful continuous organ-tones, is essentially a wide Bunsen's burner, the upper aperture of which is closed by a wire gauze which is convex on the outside. The dimensions of this burner are as follows. The vertical tube is 125 millims. in length by 17 millims. internal diameter; it fits with gentle friction on a solid cylindrical part 25 millims. in height, in which is screwed the jet for the issuing gas. At the lower part of the tube, 12 millims. from the bottom, are four rectangular orifices 12 millims. in height by 6 millims. broad, and to these correspond four slanting apertures in the massive cylinder, by which air can mix with the gas. The part where the gas emerges is 90 millims. below the top of the tube; with tap and base, the height of the entire burner is 200 millims.

To prepare the gauze cap, a disk 4 centimetres in diameter is made of iron-wire gauze of 170 to 200 meshes to the square centimetre ; this is heated, and by means of a cylindrical piece of wood of smaller diameter than the tube, with a hemispherical end, the disk is pushed into the tube, where it is held tight by friction. The top of the wire is about level with the top of the tube.

The sheet-iron pipe which is to be sounded is held by a stand in a vertical position, with its lower end about 30 millims. higher than the top of the burner (200 millims.), so that the lighted and regulated burner can be brought under the tube by means of wooden blocks, and raised in the tube to such a height that the flame is 5 to 7 centimetres above the bottom. The lower part of my stove-pipe, of 1·8 metre in length, is provided with a glass cylindrical addition, of 11 centimetres in length, so that the flame can be seen ; the tube gives the lowest F sharp of my harmonium, corresponding to ninety-four vibrations in a second.

Regulating the burner requires a little practice ; at first gas is allowed to flow freely ; it is lit, and the burner allowed to become properly hot ; the flow of gas is then regulated by means of the tap on the burner, or better by one further off, until the flame is only 2 centimetres in height. By this means a green cone of light which at first occurs in the interior of the flame gradually sinks, and is seen as a green ring of light between the gauze and the end of the tube. (Sometimes the burner now begins to buzz, and it does so without fail if the supply of gas is reduced.) If the burner is now brought into the pipe, the tube immediately begins to sound ; if not at once, the tone certainly comes when the flat hand has been held for a short time at the bottom of the tube. Instead of the hand, a pasteboard disk might be placed under the tube of the burner ; but the tone becomes thereby somewhat rough.

With a little practice in the delicate adjustment of the gas-supply, it is always possible to produce this tone and keep it constant for any length of time, even without the disk. With a normal tone the gauze is permanently incandescent ; in the dark it looks like a strawberry which is surrounded by a green ring of light ; a pencil of pale violet light rises a few millimetres above the tube.

The tone thus produced, owing to the number of accompanying harmonic upper tones, is richer than that produced by almost any other musical instrument. The simple tone of the siren is dull and thin in comparison. It can scarcely be heard without suggesting the question whether a gas-organ could not be constructed on this principle. There could scarcely be a more powerful mouthpiece for sounding a vertical open tube than such a burner ; but the certainty of starting or introducing the burner is not great enough ; the buzzing of the burner outside the tube may also be disturbing. Yet I think that by simultaneous intonation of three large pipes tuned in harmony, a powerful effect would be produced.

2. If the vibrating flame be observed in the dark in the rotating mirror, or in one which is rapidly moved backwards and forwards in the hand, the incandescent gauze gives a dark-red band, in which

bright-green rings appear at equal distances. On closer inspection, it is seen that from ring to ring a faint blue band stretches above the red one in a wave-shaped form, corresponding to the upwards and downwards flickering of the upper part of the flame. The greatest breadth of the blue band is in the neighbourhood of the green rings. The sharpness with which the latter appear in the mirror, indicates that the intervals of time during which the flame emits intense green light are small as compared with the duration of an entire pulsation. The beautiful green light does not arise from the copper of the brass tube; for it appears even in a glass burner; moreover spectral observation shows that the green of the hydrocarbon spectrum is here concerned.

3. The tube, in the condition suitable for sounding the pipe, may also be used for another experiment. If a platinum wire bent rectangularly be held vertically in the flame so that it is near the gauze, the wire soon begins to be incandescent; for the small flame is very hot, and silver readily melts in it. If now the india-rubber gas-tube be squeezed, the flame is extinguished and the wire ceases to ignite. If after a few seconds gas be again admitted, the wire again begins to glow without the gas being lighted. But when the wire is brightly glowing, the rekindling of the flame can be effected by gently breathing on it. If the flow of gas be increased, the cooling action of the gas preponderates in the lower parts of the wire, the ignited part ascends, but by diminishing the flow of gas can be depressed.

Such a straight platinum wire, glowing after the flame has been extinguished, might in many experiments serve as source of light, provided care were taken to cut off currents of air. In the form of a spiral, the wire would furnish a luminous source of heat, such as is usually furnished by a spirit-lamp. Even flat platinum strips, 12 millims. in breadth, remain ignited for a length of several centimetres after putting out the flame; cylindrically bent foil, of about 10 millims. diameter and 50 millims. height, suspended over the gauze, would perhaps be better. With a spiral and with foil, the burner may have a stronger current of gas.—Poggendorff's *Annalen*, January 1866.

NOTE ON THE MECHANICAL EQUIVALENT OF LIGHT.

BY MOSES G. FARMER, ELECTRICAL ENGINEER.

In the October Number of the Philosophical Magazine is copied from Poggendorff's *Annalen* an article by Prof. J. Thomsen of Copenhagen, "On the Mechanical Equivalent of Light."

His results show that about 13.1 foot-lbs. per minute represent the mechanical equivalent of a spermaceti candle burning at the rate of $126\frac{1}{2}$ grs. per hour.

On the evening of the 4th of July, 1863, there was exhibited by Mr. E. S. Ritchie, from the cupola of the State House in Boston, an electrical light developed by the action of 250 Bunsen cells arranged in five rows of fifty cells.

The intensity of this light was estimated by Professor William

B. Rogers, by direct measurement, as equal to that of from 10,000 to 13,000 standard candles.

If we consider the electromotive force of a Bunsen cell equal to the evolution of one cubic centimetre of mixed oxygen and hydrogen gases per minute, in a circuit of which the total resistance is equal to that of 4400 feet of a round wire $\frac{1}{20}$ th of an inch in diameter, made from electrolytic copper, and if we assume also the internal resistance of such a cell to be equal to 15 feet of such wire (these are about the average measurements), then the maximum available electrical energy which these 250 cells would evolve would be

$$\frac{1}{2} \times \frac{(n\varepsilon)^2}{2 \times \frac{nr}{m}} = \frac{1}{2} \times \frac{(50 \times 4400)^2}{2 \times \frac{50 \times 15}{5}} = 80666666.$$

Since about 614 of these units of electrical energy are the equivalent of one unit of mechanical energy, we find $\frac{80666666}{614} = 131,000$

foot-lbs. as the mechanical energy equivalent to the light developed.

Dividing this by the estimated amount of light, we get

$$\frac{131000}{10000} = 13.1, \text{ or } \frac{131000}{13000} = 10.1 \text{ foot-lbs.}$$

per minute as the mechanical equivalent of a candle-light, a remarkably close agreement with the results of Professor Thomsen.—Silliman's *American Journal* for March 1866.

ON THE COMPOSITION OF FORCES.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Belfast, April 12th, 1866.

Since the publication of the April Number of the Philosophical Magazine, in which you did me the honour to insert my communication "On the Composition of Forces," I have perceived that the demonstration of (4) proposed in the last paragraph of the note at foot of page 251 is inconclusive, as Prop. II. is not applicable to the case of the two pairs of forces referred to in that paragraph of the note; I therefore beg leave to withdraw that paragraph. But the proof of the same subsection (4) of Prop. IV., given above in the text, page 250, is unexceptionable and not much longer; and therefore this method of completing Laplace's proof is, I conceive, still perfect.

I have the honour to be,

Your obedient Servant,

JOHN STEVELLY.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JUNE 1866.

LXI. *On the Spectra of some of the Fixed Stars.* By WILLIAM HUGGINS, F.R.A.S., and W. A. MILLER, M.D., LL.D., Treas. and V.P.R.S., Professor of Chemistry, King's College, London*.

[With Two Plates.]

§ I. *Introduction.*

1. **T**HE recent discovery by Kirchhoff of the connexion between the dark lines of the solar spectrum and the bright lines of terrestrial flames, so remarkable for the wide range of its application, has placed in the hands of the experimentalist a method of analysis which is not rendered less certain by the distance of the objects the light of which is to be subjected to examination. The great success of this method of analysis as applied by Kirchhoff to the determination of the nature of some of the constituents of the sun, rendered it obvious that the application of this new method of analysis to the light which reaches the earth from the fixed stars would be an investigation of the highest interest, in relation to our knowledge of the general plan and structure of the visible universe. Hitherto the knowledge possessed by man of these immensely distant bodies has been almost confined to the fact that some of them, which observation shows to be united in systems, are composed of matter subjected to the same laws of gravitation as those which rule the members of the solar system. To this may be added the high probability that they must be self-luminous bodies analogous to our sun, and probably in some cases even transcending it in brilliancy. Were they not self-luminous, it would be impossible for their light to reach us from the enormous distances at

* From the Philosophical Transactions for 1864, Part II. Communicated, with additional notes, by the Authors.

which, as the absence of sensible parallax in the case of most of them shows, they must be placed from our system.

The investigation of the nature of the fixed stars by a prismatic analysis of the light which comes to us from them, however, is surrounded with no ordinary difficulties. The light of the bright stars, even when concentrated by an object-glass or speculum, is found to become feeble when subjected to the large amount of dispersion which is necessary to give certainty and value to the comparison of the dark lines of the stellar spectra with the bright lines of terrestrial matter. Another difficulty, greater because it is in its effect upon observation more injurious, and is altogether beyond the control of the experimentalist, presents itself in the ever-changing want of homogeneity of the earth's atmosphere through which the stellar light has to pass. This source of difficulty presses very heavily upon observers who have to work in a climate so unfavourable in this respect as our own. On any but the finest nights the numerous and closely approximated fine lines of the stellar spectra are seen so fitfully that no observations of value can be made. It is from this cause especially that we have found the inquiry, in which for more than two years and a quarter we have been engaged, more than usually toilsome; and indeed it has demanded a sacrifice of time very great when compared with the amount of information which we have been enabled to obtain.

2. Previously to January 1862, in which month we commenced these experiments, no results of any investigation undertaken with a similar purpose had been published. With other objects in view, two observers had described the spectra of a few of the brighter stars, viz. Fraunhofer in 1823*, and Donati, whose memoir, "*Intorno alle striae degli spettri stellari*," was published in the *Annali del Museo Fiorentino* for 1862.

Fraunhofer recognized the solar lines D, E, *b*, and F in the spectra of the Moon, Venus, and Mars; he also found the line D in Capella, Betelgeux, Procyon, and Pollux; in the two former he also mentions the presence of *b*. Castor and Sirius exhibited other lines. Donati's elaborate paper contains observations upon fifteen stars; but in no case has he given the positions of more than three or four bars, and the positions which he ascribes to the lines of the different spectra relatively to the solar spectrum do not accord with the results obtained either by Fraunhofer or by ourselves. As might have been anticipated from his well-known accuracy, we have not found any error in the positions of the lines indicated by Fraunhofer.

3. Early in 1862 we had succeeded in arranging a form of apparatus in which a few of the stronger lines in some of the

* Gilbert's *Annalen*, vol. lxxiv. p. 374.

brighter stars could be seen. The remeasuring of those already described by Fraunhofer and Donati, and even the determining the positions of a few similar lines in other stars, however, would have been of little value for our special object, which was to ascertain, if possible, the constituent elements of the different stars. We therefore devoted considerable time and attention to the perfecting of an apparatus which should possess sufficient dispersive and defining power to resolve such lines as D and *b* of the solar spectrum. Such an instrument would bring out the finer lines of the spectra of the stars, if in this respect they resembled the sun. It was necessary for our purpose that the apparatus should further be adapted to give accurate measures of the lines which should be observed, and that it should also be so constructed as to permit the spectra of the chemical elements to be observed in the instrument simultaneously with the spectra of the stars. In addition to this, it was needful that these two spectra should occupy such a position, relatively to each other, as to enable the observer to determine with certainty the coincidence or non-coincidence of the bright lines of the elements with the dark lines in the light from the star.

Before the end of the year 1862 we had succeeded in constructing an apparatus which fulfilled part of these conditions. With this some of the lines of the spectra of Aldebaran, α Orionis, and Sirius were measured; and from these measures diagrams of these stars, in greater detail than had then been published, were laid before the Royal Society in February 1863. After the note was sent to the Society, we became acquainted with some similar observations on several other stars by Rutherford, in Silliman's Journal for 1863*. About the same time figures of a few stellar spectra were also published by Secchi†. In March 1863 the Astronomer Royal presented to the Royal Astronomical Society a diagram in which are shown the positions of a few lines in sixteen stars‡.

Since the date at which our note was sent to the Royal Society our apparatus has been much improved, and in its present form of construction it fulfils satisfactorily several of the conditions required.

§ II. *Description of the Apparatus and Methods of Observation employed.*

4. This specially constructed spectrum-apparatus is attached to the eye-end of a refracting telescope of 8 inches aperture and 10 feet focal length, which is mounted equatorially in the obser-

* Vol. xxxv. p. 71.

† *Astronomische Nachrichten*, No. 1405, March 3, 1863.

‡ Monthly Notices of the Royal Astronomical Society, vol. xxiii. p. 190.

vatory of Mr. Huggins at Upper Tulse Hill. The object-glass is a very fine one, by Alvan Clark of Cambridge, Massachusetts; the equatorial mounting is by Cooke of York; and the telescope is carried very smoothly by a clock motion.

As the linear spectrum of the point of light which a star forms at the focus of the object-glass is too narrow for the observation of the dark lines, it becomes necessary to spread out the image of the star; and to prevent loss of light, it is of importance that this enlargement should be in one direction only; so that the whole of the light received by the object-glass should be concentrated into a fine line of light as narrow as possible, and having a length not greater than will correspond to that breadth of the spectrum which (when viewed in the apparatus) will be just sufficient to enable the eye to distinguish with ease the dark lines by which it may be crossed. No arrangement tried by us has been found more suitable to effect this enlargement in one direction than a cylindrical lens, which was first employed for this purpose by Fraunhofer. In the apparatus by which the spectra described in our "Note" of February 1863 were observed, the cylindrical lens employed was plano-convex, of 0.5 inch focal length. This was placed within the focus of the object-glass and immediately in front of the slit of the collimator.

The present form of the apparatus is represented in Plate V. figs. 1 and 2, where the cylindrical lens is marked *a*. This is plano-convex, an inch square, and of about 14 inches focal length. The lens is mounted in an inner tube, *b*, sliding within the tube *c*, by which the apparatus is adapted to the eye-end of the telescope. The axial direction of the cylindrical surface is placed at *right angles* to the slit *d*, and the distance of the lens from the slit within the converging pencils from the object-glass is such as to give exactly the necessary breadth to the spectrum.

In consequence of the object-glass being over-corrected, the red and, especially, the violet pencils are less spread out than the pencils of intermediate refrangibility; so that the spectrum, instead of having a uniform breadth, becomes slightly narrower at the red end, and tapers off in a greater degree towards the more refrangible extremity*.

* The experiment was made, of so placing the cylindrical lens that the axial direction of its convex cylindrical surface should be *parallel* with the direction of the slit. The line of light is in this case formed by the lens; and the length of this line, corresponding to the visible breadth of the spectrum, is equal to the diameter of the cone of rays from the object-glass where they fall upon the slit. With this arrangement, the spectrum appears to be spread out, in place of being contracted, at the two extremities. Owing to the large amount of dispersion to which the light is subjected, it was judged unadvisable to weaken still further the already feeble illumination of the extremities of the spectrum; and in the examination of the

In front of the slit *d*, and over one half of it, is placed a right-angled prism *e*, for the purpose of reflecting the light which it receives from the mirror *f* through the slit. In the brass tube *c* are two holes: by one of these the light is allowed to pass from the mirror to the reflecting-prism *e*; and by means of the other, access to the milled head for regulating the width of the slit is permitted. Behind the slit, and at a distance equal to its focal length, is placed an achromatic collimating lens *g*, made by T. Ross; this has a diameter of 0.6 inch, and a focal length of $4\frac{1}{2}$ inches. These proportions are such that the lens receives the whole of the light which diverges from the linear image of the star when this is brought exactly within the jaws of the slit.

The dispersing portion of the apparatus consists of two prisms, *h*, each having a refracting angle of about 60° ; they were made by T. Ross, and are of very dense and homogeneous flint glass. The prisms are supported upon a suitable mounting, which permits them to be duly levelled and adjusted. Since the feebleness of the light from the stars limits the observations for the most part to the central and more luminous portions of the spectrum, the prisms have been adjusted to the angle of minimum deviation for the ray D. A cover of brass, *k*, encloses this part of the apparatus; and by this means the prisms are protected from accidental displacement and from dust.

The spectrum is viewed through a small achromatic telescope *l*, furnished with an object-glass of 0.8 inch diameter and 6.75 inches focal length. This telescope has an adjustment for level at *m*. The axis of the telescope can be lowered and raised, and the tube can be also rotated around the vertical axis of support at *n*. At the focus of the object-glass are fixed two wires, crossing at an angle of 90° . These are viewed, together with the spectrum, by a positive eyepiece *p*, giving a magnifying power of 5.7 diameters. As the eyes of the two observers do not possess the same focal distance, a spectacle-lens, corresponding to the focal difference between the two, was fitted into a brass tube, which slipped easily over the eyepiece of the telescope, and was used or withdrawn as was necessary.

This telescope, when properly adjusted and clamped, is carried by a micrometer-screw *q*, which was constructed and fitted to the instrument by Cooke and Sons. The centre of motion about which it is carried is placed approximatively at the point of in-

stellar spectra the position of the cylindrical lens with its axis at right angles to the slit, as mentioned in the text, was therefore adopted.

A *plano-concave* cylindrical lens of about 14 inches negative focal length was also tried. The slight advantage which this possesses over the convex form is more than balanced by the inconvenience of the increased length given to the whole apparatus.

tersection of the red and the violet pencils from the last prism ; consequently it falls within the last face of the prism nearest the small telescope. All the pencils therefore which emerge from the prism are, by the motion of the telescope, caused to fall nearly centrically upon its object-glass. The micrometer-screw has 50 threads to an inch ; and each revolution is read to the hundredth part, by the divisions engraved upon the head. This gives a scale of about 1800 parts to the interval between the lines A and H of the solar spectrum. During the whole of the observations the same part of the screw has been used ; and the measures being relative, the inequalities, if any, in the thread of this part of the screw do not affect the accuracy of the results. The eye-lens for reading the divisions of the micrometer-screw is shown at *s*.

The mirror *f* receives the light to be compared with that of the star-spectrum, and reflects it upon the prism *e*, in front of the slit *d*. This light was usually obtained from the induction-spark taken between electrodes of different metals, fragments or wires of which were held by a pair of small forceps attached to the insulating ebonite clamp *r*. Upon a moveable stand in the observatory was placed the induction-coil, already described by one of us*, in the secondary circuit of which was inserted a Leyden jar having 140 square inches of tinfoil upon each of its surfaces. The exciting battery, which, for the convenience of being always available, consisted of four cells of Smee's construction, with plates 6 inches by 3, was placed without the observatory. Wires, in connexion with this and the coil, were so arranged that the observer could make and break contact at pleasure without removing his eye from the small telescope. This was the more important since, by tilting the mirror *f*, it is possible, within narrow limits, to alter the position of the spectrum of the metal relatively to that of the star. An arrangement is thus obtained which enables the observer to be assured of the perfect correspondence in relative position in the instrument of the stellar spectrum and the spectrum to be compared with it.

5. The satisfactory performance of this apparatus is proved by the very considerable dispersion and admirably sharp definition of the known lines in the spectra of the sun and metallic vapours. When it is directed to the sun, the line D is sufficiently divided to permit the line within it, marked in Kirchhoff's map as coincident with nickel, to be seen. The close groups of the metallic spectra are also well resolved.

When this improved apparatus was directed to the stars, a large number of fine lines was observed, in addition to those that had been previously seen. In the spectra of all the brighter

* Philosophical Transactions, 1864, p. 141.

stars which we have examined, the dark lines appear to be as fine and as numerous as they are in the solar spectrum. The great breadth of the lines in the green and more refrangible parts of Sirius and some other stars, as seen in the less perfect form of apparatus which was first employed, and which band-like appearance was so marked as specially to distinguish them, has, to a very great extent, disappeared; and though these lines are still strong, they now appear, as compared with the strongest of the solar lines, by no means so abnormally broad as to require these stars to be placed in a class apart. No stars sufficiently bright to give a spectrum have been observed to be without lines. The stars admit of no such broad distinctions of classification. Star differs from star alone in the grouping and arrangement of the numerous fine lines by which their spectra are crossed.

6. For the convenience of reference and comparison, a few of the more characteristic lines of twenty-nine of the elements were measured with the instrument. These were laid down to scale, in order to serve as a chart, for the purpose of suggesting, by a comparison with the lines measured in the star, those elements the coincidence of the lines of which with stellar lines was probable.

For the purpose of ensuring perfect accuracy in relative position in the instrument between the star-spectrum and the spectrum to be observed simultaneously with it, the following general method of observing was adopted:—The flame of a small lamp of alcohol saturated with chloride of sodium was placed centrally before the object-glass of the telescope, so as to furnish a sodium-spectrum. The sodium-spectrum was then obtained by the induction-spark, and the mirror *f* was so adjusted that the components of the double line D, which is well divided in the instrument, should be severally coincident in the two spectra. The lamp was then removed, and the telescope directed to the sun, when Fraunhofer's line D was satisfactorily observed to coincide perfectly with that of sodium in the induction-spark. Having thus ascertained that the sodium lines coincided in the instrument with the solar lines D, it was of importance to have assurance from experiment that the other parts of the solar spectrum would also accurately agree in position with those corresponding to them in the spectrum of comparison. When electrodes of magnesium were employed, the components of the triple group characteristic of this metal severally coincided with the corresponding lines of the group *b*. C and F also agreed exactly in position with the lines of hydrogen; the coincidence of several of the principal lines of iron was also observed. The stronger of the Fraunhofer lines were measured in the spectra of the moon and of Venus, and these measures were found to be

accordant with those of the same lines taken in the solar spectrum.

Before commencing the examination of the spectrum of a star, the alcohol-lamp was again placed before the object-glass of the telescope, and the correct adjustment of the apparatus obtained with certainty. The first observation was whether the star contained a double line coincident with the sodium lines D. When the presence of such a line had been satisfactorily determined, we considered it sufficient in subsequent observations of the same star to commence by ascertaining the exact agreement in position of this known stellar line with the sodium lines D.

Since from flexure of the parts of the spectrum-apparatus the absolute reading of the micrometer might vary when the telescope was directed to stars differing greatly in altitude, the measure of the line in the star which was known to be coincident with that of sodium was always taken at the commencement and at the end of each set of measures. The distances of the other lines from this line, and not the readings of the micrometer, were then finally registered as the measures of their position; and these form the numbers given in the Tables, from which the diagrams of the star-spectra have been laid down.

The very close approximation*, not unfrequently the identity, of the measures obtained for the same line on different occasions, as well as the very exact agreement of the lines laid down from these measures with the stellar lines subsequently determined by a direct comparison with metallic lines the positions of which were known, have given the authors great confidence in the minute accuracy of the numbers and drawings which they have now the honour of laying before the Society.

§ III. *Observations on the Moon and Planets.*

7. It is well known that in the solar spectrum many additional remarkable lines make their appearance when light from the sun seen near the horizon reaches the observer, after having traversed a much greater length of our atmosphere than when the sun is viewed at greater altitudes. This circumstance suggested to us the importance of a careful examination of the solar light after reflexion from the moon and planets, in reference to the extent and analogous constitution of atmospheres possibly surrounding these bodies. As far as practicable, the spectra of the moon, Venus, Mars, Jupiter, and Saturn have been observed on several occasions with this special object in view.

8. *The Moon.*—All the astronomical phenomena in which we

* These measures, on repeated observation, seldom varied more than a single division of the scale, or $\frac{1}{1800}$ th of the distance between A and H.

should expect to discover indications of an atmosphere about the moon, if such exist, agree in proving the non-existence of a lunar atmosphere of sensible amount. From the absence of appreciable refraction at the moon's limb, and from the *sudden* extinction during a total lunar eclipse of stars of even the tenth and eleventh magnitude at the limb of the moon, "we are," writes Sir John Herschel, "entitled to conclude that no amount of appreciable vapour is suspended near the surface of the moon, and . . . the non-existence of an atmosphere at the moon's edge having the 1980th part of the density of the earth's atmosphere"*.

As by direct observation we know that the solar light is reflected from the *surface* of the moon, the light which reaches the earth after having undergone this reflexion must have passed through a length of lunar atmosphere, if such exist, at least equal to double the height of such atmosphere above that surface of the moon which is visible to us. From some parts of the moon, when the whole or a large part of its illuminated surface is turned towards the earth, the length of the column of lunar atmosphere which the solar light would have to traverse would be considerably greater.

The examination of lunar light by the spectroscope, and the comparison of the light reflected from different portions of the moon's illuminated surface with each other by this method, would take place under conditions favourable to the detection of an atmosphere of considerable extent, if such exist.

The moon was examined by us on April 12 and November 26, 1862, March 31 and December 31, 1863, March 15 and 19, and April 12, 1864. The solar lines were perfectly well seen, appearing exceedingly sharp and fine. The line D was well divided, and its components were observed to coincide with those of sodium. Coincidence of the magnesium group with the three lines forming *b* was also observed. The lunar spectrum is indeed full of fine lines; and they were well seen, from B to about half-way between G and H. On all these occasions no other strong lines were observed than those which are visible in the solar spectrum when the sun has a considerable altitude.

Previously to the observations of March 15 and 19, and April 12, 1864, the apparatus was directed to the sun when near the horizon, and the relative positions and characteristic appearances of the atmospheric lines in the orange and red were carefully observed. These portions of the spectrum were closely scrutinized when the moon's light was afterwards examined; but no

* Outlines of Astronomy, 7th edition, par. 431, p. 284. See also a paper by Professor Challis in the Monthly Notices of the Royal Astronomical Society, vol. xxiii. p. 254, June 1863.

indication of similar lines could be detected. On each of the three evenings just mentioned successive portions of the moon's illuminated surface, from the centre to the circumference, were brought before the slit of the spectrum-apparatus. The quantity of light from different parts was observed to be very different; but not the smallest change in the lines of the spectrum could be perceived, either in respect of relative intensity or the addition or disappearance of any lines*.

The result of this spectrum analysis of the light reflected by the moon is wholly negative as to the existence of any considerable lunar atmosphere†.

9. *The Planets Jupiter, Saturn, Mars, and Venus.*—The very rapidly changing appearances of the disk of Jupiter, other than those due to the rotation of the planet, present very strong evidence of the existence of a very considerable atmosphere

* [With the spectrum-apparatus described at page 415, the spectra of particular and very limited regions of the moon's surface can be examined. The opening of the slit of the apparatus corresponding to a spectrum that can be separately observed is about $\frac{1}{300}$ inch \times $\frac{1}{100}$ inch. The image of the moon formed by the object-glass of the telescope has a diameter of 1.04 inch. Practically it is found that the light reflected from an area upon the surface of the moon of about one-third that of Tycho can be analyzed in the instrument.

The particular spot of the moon's surface under observation can be ascertained by means of the finder attached to the telescope. For this purpose, however, a special set of wires, accurately adjusted, and an eyepiece of considerable power are necessary. When the part of the moon's surface under observation presents marked inequalities of illumination, the spectra of these differently illuminated portions can be easily recognized by the differences in their comparative brightness. In these observations the cylindrical lens may be removed.—August 31, 1864.]

† [A remarkably favourable opportunity of observing the effect upon the solar spectrum of transmission through a very large extent of the earth's atmosphere presents itself on the occasion of an eclipse of the moon. We had made preparations to observe the copper-coloured light reflected from the moon during the eclipse of June 1, 1863. The small altitude of the moon on this occasion rendered the observation impossible, from the circumstance that the eye-end of the telescope, increased in length by the spectrum-apparatus, came too near the wall of the observatory.—August 31, 1864.]

The authors observed the partial eclipse of the moon on October 4, 1865. The spectrum of the penumbra appeared singularly shortened at its more refrangible end. The atmospheric groups near D were distinctly seen in the light of the penumbra.

One of the authors observed the disappearance of the spectrum of ϵ Piscium at its occultation on January 4, 1866. No dark lines additional to those belonging to the star were seen as the moon's dark limb approached and occulted the star. The disappearance of the spectrum was not preceded by any sensible failure of the blue or red rays, but the spectrum appeared to remain unaltered in the relative intensity of its different parts up to the moment of extinction. (Monthly Notices of the Royal Astronomical Society, vol. xxv. p. 60.)]

about Jupiter. The same, though in a much less marked degree, is probably true of Saturn and Mars. In addition, the diminished brightness of the disk of Jupiter near the periphery supports the inference that an atmosphere exists about that planet.

The planet *Jupiter* was observed on April 12, 1862, and April 14 and May 1, 1863. The solar lines B, C, D, E, *b*, F, and G were seen, with numerous fine intermediate lines, and D, E, *b*, and F were measured; but no marked lines other than those usually present in the solar spectrum were detected.

[Since these observations were made, we have had a spectrum-apparatus constructed by Mr. Browning, optician, of the Minories, which is similar in general arrangement to that already described, but possesses much less dispersive power. In this apparatus the cylindrical lens, the collimating lens, and the object-glass of the small telescope correspond exactly in diameter and in focal length with those of which a description has been given; but the eyepiece of the telescope is of less power, and has a magnifying-power of about three diameters. A second eyepiece was occasionally used, magnifying nine diameters. Two prisms are employed; one has a refracting angle of 35° , the other a refracting angle of 45° .

With this apparatus, in the spectrum of Jupiter a strong line in the red is seen which is scarcely distinguishable with the more powerful instrument, and was from this cause overlooked in our earlier observations. The remarkable increase of visibility of this line is due to the much greater brilliancy of the spectrum in this apparatus; and this is much more than inversely proportional to the diminution of the dispersion, since, on account of the greatly reduced obliquity of incidence, the loss of light at the surfaces of the prisms by reflexion is much less. This saving of light in the spectrum-apparatus is of very great importance in observations of the planetary spectra. The image of a planet in the telescope is not a point, but forms a disk of considerable magnitude relatively to the image of a star. Of this image, enlarged in one direction by the cylindrical lens, a very narrow section only, corresponding to the breadth of the slit, passes on through the collimating lens to the prisms; and this portion only of the total light collected by the object-glass becomes available to form the spectrum. On this account we have found the observations of the planets much more difficult than would be observations of stars possessing an equal apparent brilliancy.

This band of which we are now speaking in the spectrum of Jupiter occurs in a rather obscure part of the spectrum; moreover, by the instrument of greater dispersive power, it appears to be resolved into two or more lines, which are severally very faint, and are less visible than a single stronger line. The alti-

tude of Jupiter being small (about 22° above the horizon) at the time of observation, it was of great importance to have satisfactory evidence that this band was not due to absorption by our atmosphere.

On June 16, 1864, the moon and Jupiter being near each other in the sky, the opportunity was seized to compare directly the moon's light with that of Jupiter under precisely similar conditions of atmosphere. The observations of this evening were decisive in showing that this band in the spectrum of Jupiter was due to a modification suffered by the solar light before reaching our atmosphere, and therefore due probably to absorption by the atmosphere of Jupiter.

On June 20, and on July 12 and 14, an observation still more crucial was obtained. The length of the opening of the slit is much greater than the diameter of the telescopic image of Jupiter, even after elongation by the cylindrical lens. If, therefore, at the time of observation the light from the sky is sufficiently intense to form a visible spectrum, the spectrum of the sky is seen in the instrument together with the spectrum of Jupiter, and much exceeding it in breadth. When the period is so chosen that the degree of illumination of the sky is suitable in proportion to the intensity of the light of Jupiter, the solar lines and those due to our atmosphere are well seen in close contiguity with the lines in the spectrum of Jupiter, and occupying exactly similar relative positions. The sky-spectrum is seen under precisely similar conditions of altitude and of state of atmosphere. To the light of Jupiter under these circumstances of observation is added the light reflected from the small area of sky immediately between the observer and the planet. This light, however, is too faint in proportion to that of Jupiter to become a source of error. In the diagram, fig. 3, Plate V., the position of this band is shown relatively to the spectrum of the sky. The band at 914 of the scale appears to be coincident with, but *much stronger* than, a faint band in the sky-spectrum. This increase in the strength of the band is probably due to an absorptive action exerted by the atmosphere of the planet.

The bands at 882 and 1033 of the scale are less intense in the spectrum of Jupiter than in the spectrum of the light of the sky. This variation of intensity is probably due to the circumstance that the light from the southern sky, before it is reflected to the observer, on account of the position of the sun, which is then near the horizon, has had to traverse a very much larger amount, and a more dense portion, of our atmosphere than that traversed by the light received from Jupiter. It is in accordance with this explanation that these bands are also less intense in the spectrum of the moon when similarly compared with those of the sky.

Other lines less refrangible were perceived in the spectrum of Jupiter, but were not sufficiently distinct to be measured. The bands in the orange and the red to which we have referred, when examined in the spectrum-apparatus of greater dispersive power, and with a much stronger illumination by directing the apparatus to the sun when near the horizon, are resolved into groups of lines. The stronger of these lines are represented in the upper spectrum of the diagram. The relative position of the band in the red due to lines of oxygen and nitrogen when the induction-spark is taken in air, is shown below the spectrum of Jupiter. This band is in a small degree more refrangible than the strong band due to Jupiter.

If this band, at 914 of the scale, in Jupiter's spectrum consists of lines severally coincident with the lines composing the faint atmospheric band with which it appears to correspond in position, it would seem entitled to be regarded as an evidence of the similarity of Jupiter's atmosphere to our own, with respect at least to one of its constituents, or to one of the vapours diffused through it. The smaller intensity of the bands 882 and 1033 would appear to oppose the supposition that Jupiter's atmosphere is identical with our own. This negative evidence, however, cannot be regarded as of much weight, since telescopic observations show that the light which we receive from Jupiter is for the most part reflected from clouds floating in its atmosphere at an elevation above the planetary surface. The solar light, therefore, would not traverse the lower and denser portions of Jupiter's atmosphere, corresponding to those of our own atmosphere in which the vapours, which probably produce these lines, appear to be chiefly present. The band about C, and that a little more refrangible at 838 of the scale, appear quite as strong in Jupiter as in the light from the sky. It may therefore be supposed that these bands are in part due to absorption at Jupiter, since the light from Jupiter suffers less absorption from our atmosphere than does the solar light reflected from the sky under the circumstances in which the observations were made.

With the exception of these bands in the orange and the red, the spectrum of Jupiter appeared to correspond exactly with that of the sky.—August 31, 1864.]

Saturn was observed on April 12, 1862, April 14, 1863, and April 12, 1864. Several solar lines were seen, but the spectrum was too faint to permit of any satisfactory determination as to the presence or absence of atmospheric lines.

[The spectrum of Saturn was observed with the apparatus and in the manner described when speaking of Jupiter, on June 13, 16, and 20, 1864. The spectrum was more difficult of observation, on account of the feebler brilliancy of Saturn, and its

less favourable position. Bands in the red and orange were seen similar to those in the spectrum of Jupiter, and by measurement these bands were found to occupy positions in the spectrum corresponding to those of the bands of Jupiter.—August 31, 1864.]*

The spectrum of *Mars* was observed on November 6, 1862, and April 17, 1863. The principal solar lines were seen, and no other strong lines were noticed.

[On August 10 and 29, 1864, we re-examined Mars, using the new spectrum-apparatus. No lines in the red, similar to those of Jupiter and Saturn, were observed; but in the extreme red, probably about B and *a*, two or three strong lines were seen. With the exception of these, no lines were detected in the red, orange, yellow, and green portions of the spectrum, other than those of the solar spectrum. At about F the brilliancy of the spectrum diminishes in a remarkable manner, in consequence of a series of strong and nearly equidistant bands, which commences at F and continues towards the more refrangible end as far as the spectrum can be traced. The absorption of these bands is evidently the cause of the predominance of the red rays in the light of this planet.

The spectrum-apparatus of greater power resolves these bands in the blue into groups of lines.—August 31, 1864.]

The light of *Venus* gives a spectrum of great beauty. The observations were chiefly made on April 17, 22, and 26, 1863. The line D was seen double. B, C, and numerous solar lines to a little distance beyond G, were distinctly visible; and the principal of these were measured and found to agree with corresponding lines in the solar spectrum. Lines other than these, and in the position in which the stronger atmospheric lines present themselves, were carefully looked for, but no satisfactory evidence of any such lines has been obtained. Venus was observed as early in the evening as possible, and while a considerable amount of daylight still remained.

The imperfect evidence which analysis by the prism affords of the existence of atmospheres around these planets, notwithstanding the high probability, amounting almost to certainty in the case of Jupiter, that such atmospheres do exist, may receive an explanation in the supposition that the light is chiefly reflected, not from the planetary surfaces, but from masses of cloud in the upper strata of their atmospheres. In this case the length of

* [The authors reobserved Saturn on June 13, 1865. They found the spectra of the ansæ of the rings to be brighter than that of the ball. The dark lines in the red and orange, however, were less distinct in the light of the rings than in that of the ball. This observation is in accordance with the telescopic appearance of Saturn and his rings when viewed with high powers.]

atmosphere which the light would have to traverse would be considerably lessened. With perhaps the exception of Mars, telescopic observations are in favour of such a supposition.

§ IV. *Observations on the Fixed Stars.*

10. The number of fixed stars which we have, to a greater or less extent, examined amounts to nearly 50. We have, however, concentrated our efforts upon three or four of the brighter stars; and two only of these have been mapped with any degree of completeness. These spectra are, indeed, as rich in lines as that of the sun; and even with these it may be advantageous to compare the spectra of additional metals when the season is again favourable. The few really fine nights that are available whilst the star is well situated for such observations, in respect of altitude and the time of sun-setting, necessarily make the *complete* investigation even of a single star the work of some years.

11. Aldebaran (α Tauri) (Plate VI.).—The light of this star is of a pale red. When viewed in the spectroscope, numerous strong lines are at once evident, particularly in the orange, the green, and the blue portions. The positions of about seventy of these lines have been measured, and their places have been given in the Table. Besides these, numerous other strong lines are visible, particularly in the blue; but they have not been measured, owing to the feebleness of the light; we have therefore not inserted them in the Table or in the diagram. A similar remark is applicable also to the results of our examination of α Orionis and β Pégasi.

We have compared the spectra of sixteen of the terrestrial elements by simultaneous observation with the spectrum of Aldebaran, of course selecting those in which we had reason, from the observations, to believe coincidence was most likely to occur. Nine of these spectra exhibited lines coincident with certain lines in the spectrum of the star. They are as follows:—sodium, magnesium, hydrogen, calcium, iron, bismuth, tellurium, antimony, and mercury.

(1) *Sodium*.—The double line at D was coincident with a double line in the stellar spectrum.

(2) *Magnesium*.—The three components of the group at *b*, from electrodes of the metal, were coincident with three lines in the star-spectrum.

(3) *Hydrogen*.—The line in the red corresponding to C, and the line in the green corresponding to F in the solar spectrum, were coincident with strong lines in the spectrum of Aldebaran.

(4) *Calcium*.—Electrodes of the metal were used; four lines in its spectrum were observed to coincide with four of the stellar lines.

(5) *Iron*.—The lines in the spectrum of this metal are very numerous, but not remarkable for intensity. There was a double line corresponding to E in the solar spectrum, and three other more refrangible well-marked lines coincident with lines in the star.

(6) *Bismuth*.—Four strong lines in the spectrum of this metal coincided with four in the star-spectrum.

(7) *Tellurium*.—In the spectrum of this metal also four of the strongest lines coincided with four in the spectrum of the star.

(8) *Antimony*.—Three of the lines in the spectrum of antimony were observed to coincide with stellar lines.

(9) *Mercury*.—Four of the brightest lines in the mercury-spectrum correspond in position with four lines of the star.

It must not be supposed that other lines in the spectra of all the elements above enumerated do not possess corresponding lines in the star-spectrum. Comparisons of this kind are extremely fatiguing to the eye, and are necessarily limited to the stronger lines of each spectrum. In no case, in the instances above enumerated, did we find any strong line in the metallic spectrum wanting in the star-spectrum, in those parts where the comparison could be satisfactorily instituted.

Seven other elements were compared with this star, viz. nitrogen, cobalt, tin, lead, cadmium, lithium, and barium. No coincidence was observed. With *nitrogen* three strong double lines were compared, with *cobalt* one strong single line and a double line, with *tin* five lines, with *lead* two strong lines, with *cadmium* three lines, with *barium* two of the strongest in the green, and with *lithium* the line in the orange, but were found to be without any strong lines in the star-spectrum corresponding with them. The positions of these several lines relatively to the star-spectrum are given in the diagram.

12. α Orionis (Betelgeux) (Plate VI.).—The light of this star has a decided orange tinge. None of the stars which we have examined exhibits a more complex or remarkable spectrum than this. Strong groups of lines are visible, especially in the red, the green, and the blue portions. In the blue comparatively few of these lines have been measured with accuracy; we have therefore not inserted them in the Table or the diagram. We have measured the position of about eighty lines in the brighter portions of this spectrum.

In the interval between the divisions 890 and 920 of the scale adopted in the diagram, is a shading as of fine lines. A fainter shading of the same character is observed between 990 and 1010, also from 1050 to 1069. A stronger similar shading occurs

from 1145 to 1170, and from 1280 to 1300. A similar shaded band commences at 1420, and another at 1557*.

The spectra obtained from sixteen elementary bodies were observed simultaneously with the spectrum of α Orionis. In five of these, viz. sodium, magnesium, calcium, iron, and bismuth, lines corresponding with certain stellar lines were found to exist.

(1) *Sodium*.—The lines coincident with D are fainter in this star than in Aldebaran.

(2) *Magnesium*.—Decided group of three stellar lines coincident with the group at *b*.

(3) *Calcium*.—Four lines of this metal were on two different occasions seen to be coincident with four lines in the spectrum of the star.

(4) *Iron*.—The double line of this metal at E, and three other more refrangible bright lines, coincide with lines in the star-spectrum.

(5) *Bismuth*.—In the spectrum of this metal also, four lines were found to coincide with four in the stellar spectrum.

Thallium.—The bright green line so characteristic of this metal appears to coincide with one of the lines seen in the star-spectrum; but this line may be due to calcium, since the small difference between the position of the thallium line and that of one of the calcium lines very close to it would not be distinguishable with the dispersive power of the apparatus employed.

In the spectra of the other elements which we compared with that of the star, no coincidences occur.

Hydrogen.—There is no line coincident with the red line C of hydrogen; but in the star are two strong lines, one on either side of the position of C: there is also no line coincident with F. It is strikingly confirmatory of this method of analysis, that in all the stars hitherto examined by us in which a line corresponding to C exists, that corresponding to F is also found. When F is absent, C is also wanting.

In *nitrogen* three strong double lines were compared. In *tin*

* [α Orionis has long been known as a variable star of great irregularity, both of period and of extent of change of brightness. The authors found, when they reobserved the spectrum of this star in February and March 1866, that the shading as of fine lines, terminated at its more refrangible end by the strong line 1069.5, was not then to be seen. The absence of this group is of great interest in connexion with the variability of the star's light, especially as the time of the disappearance of this group coincides with the epoch of the maximum brilliancy of the star. The variation in the colour of the star's light observed by Mr. Baxendell is such as would be produced by the absence or presence of this group. The authors have been engaged with observations of other variable stars. (Monthly Notices of the Royal Astronomical Society, vol. xxvi. p. 217.)]

five lines, and in *lead* two bright lines were compared, but no coincidence was found.

Gold.—The strongest of the gold lines approximates closely in position to one in the spectrum of the star, but it is probably not coincident.

Three of the strong lines of *cadmium*, two of *silver*, four of *mercury*, two of *barium*, and one (the orange line) of *lithium* were observed to be not coincident with any of the lines visible in the star. In these comparisons, when barium was used, it was employed in the form of a nearly solid amalgam.

The opening of the slit was maintained at the same width (not more than the $\frac{1}{500}$ th of an inch) for all the observations, both with Aldebaran and α Orionis. In the case of the fainter star which follows, it was very slightly widened.

13. β PEGASI.—The colour of this star is a fine yellow. In the general arrangement of the groups, in the gradation of the strength of the lines composing the groups, and in the absence of the hydrogen lines, this spectrum, though much fainter, is closely analogous with the spectrum of α Orionis, as figured in the Plate.

This star was carefully observed on many different occasions; but the faintness of the star, and the unfavourable state of the atmosphere on many of the nights of observation, did not permit the same number of lines to be measured, or allow a comparison with an equal number of terrestrial elements. From November 10, 1862, when twelve lines were observed, to the present year, we have scrutinized the star carefully.

Nine of the elements were compared with the spectrum of β Pegasi. Two of these, viz. *sodium* and *magnesium*, and perhaps a third, viz. *barium*, furnish spectra in which there are lines which coincide with lines in the spectrum of the star.

The spectra of *iron* and *manganese* were also compared with that of the star, but the state of the atmosphere prevented any certain conclusion.

The lines in the spectra of *nitrogen*, *tin*, and *mercury* were not coincident with any definite lines in the star-spectrum. Neither of the *hydrogen* lines corresponding to C and F was present.

At the end of the paper we have given a Table of such measures of the lines in the spectrum of this star as we can depend upon. Although it appears to be as full of lines as either of the preceding stars, the observations are attended with great difficulty, owing to the insufficient amount of light.

The absence in the spectrum of α Orionis, and also in the spectrum of β Pegasi (which so closely resembles it in character), of lines corresponding to those of hydrogen, is an observation of considerable interest. It is of the more importance since the

lines C and F are highly characteristic of the solar spectrum and of the spectra of by far the larger number of the fixed stars to which our observations have been extended.

These exceptions are further interesting as they seem to prove that the lines C and F are due to the luminous bodies themselves. Of this some doubt might be entertained, and it might be suspected that they are in some way due to our own atmosphere, if these lines were present in the spectra of *all* the stars without exception.

This absence of the lines corresponding to hydrogen is also the more entitled to consideration since it is so rare to find them wanting, amongst the considerable number of stellar spectra which we have observed.

14. SIRIUS.—The spectrum of this brilliant white star is very intense; but owing to its low altitude, even when most favourably situated, the observation of the finer lines is rendered very difficult by the motions of the earth's atmosphere. For the present we do not give any details of our measures. The lines in the green and blue appeared, in the less perfect form of spectroscopic which we employed in the early part of 1863, of very great breadth, and were so figured in the diagram of the spectrum of this star given in our "Note" of February 1863. With our present instrument, possessing much greater dispersive power and a very narrow slit, these bands appear but little broader than F and G are at times seen in the solar spectrum. In February 1863, the breadth of the band corresponding to F measured $1\frac{1}{2}$ unit of the scale we then adopted; each unit corresponded to 15.5 units of our present scale. The micrometric measurement of this line in Sirius, in terms of our present scale, is only 3.7—that is, only about one-seventh of the breadth as seen with the wider slit and a dispersing arrangement having little more than one-third of the power of the present apparatus.

Three if not four elementary bodies have been found to furnish spectra in which lines coincide with those of Sirius, viz. sodium, magnesium, hydrogen, and probably iron.

(1) *Sodium*.—A double line in the star, though faint, coincides in position with the line of this metal.

(2) *Magnesium*.—Three lines in the star-spectrum coincide with the triple group of magnesium.

(3) *Hydrogen*.—Both the lines corresponding to F and C have intensely strong lines in the star-spectrum.

(4) *Iron*.—No direct comparison with this metal was made; but the cross wires having been set to a position corresponding with E of the solar spectrum, a faint line in the star was seen exactly to bisect the wires when the telescope was turned upon Sirius.

The whole spectrum of Sirius is crossed by a very large number of faint and fine lines.

It is worthy of notice that in the case of Sirius, and a large number of the white stars, at the same time that the hydrogen lines are abnormally strong as compared with the solar spectrum, all the metallic lines are remarkably faint.

On the 27th of February, 1863, and on the 3rd of March of the same year, when the spectrum of this star was caused to fall upon a sensitive collodion surface, an intense spectrum of the more refrangible part was obtained. From want of accurate adjustment of the focus, or from the motion of the star not being exactly compensated by the clock movement, or from atmospheric tremors, the spectrum, though tolerably defined at the edges, presented no indications of lines. Our other investigations have hitherto prevented us from continuing these experiments further; but we have not abandoned our intention of pursuing them.

15. α LYRÆ (Vega).—This is a white star having a spectrum of the same class as Sirius, and as full of fine lines as the solar spectrum. Many of these we have measured, but our investigation of this star is incomplete.

We have ascertained the existence, in the stellar spectrum, of a double line at D corresponding to the lines of *sodium*, of a triple line at *b* coinciding with the group of *magnesium*, and of two strong lines coincident with the lines of *hydrogen* C and F.

16. CAPELLA.—This is a white star with a spectrum closely resembling that of our sun. The lines are very numerous; we have measured more than twenty of them, and ascertained the existence of the double *sodium* line at D, but we defer giving details until we have completed our comparison with the spectra of other metals.

From this star we obtained (on February 27, 1863) a photograph of the more refrangible end of the spectrum; but the apparatus was not sufficiently perfect to exhibit any stellar lines.

17. ARCTURUS (α Boötis).—This is a red star the spectrum of which somewhat resembles that of the sun. In this also we have measured upwards of thirty lines, and have ascertained the existence of a double *sodium* line at D; but our comparisons with other metallic spectra are not yet complete.

18. POLLUX.—In the spectrum of this star, which is rich in lines, we have measured twelve or fourteen, and have observed coincidences with the lines of *sodium*, *magnesium*, and probably of *iron*. At any rate there is a line which we believe occupies the position of E in the solar spectrum.

α CYGNI and PROCYON are both full of fine lines. In each of these spectra we observed a double line coincident with the *sodium* D.

19. The following stars have also been observed: numerous lines are seen in the spectrum of each; and in some, several of the lines were measured; but we have not yet instituted any comparisons with the metallic spectra.

Castor; ϵ , ζ , and η *Ursæ majoris*; α and ϵ *Pegasi*; α , β , and γ *Andromedæ*, the last an interesting spectrum; *Rigel*, a spectrum full of fine lines; η *Orionis*; α *Trianguli*; γ and ϵ *Cygni*; α , β , γ , ϵ , and η *Cassiopeia*; γ *Geminorum*; β *Canis majoris*; β *Canis minoris*; *Spica*; γ , δ , and ϵ *Virginis*; α *Aquilæ*; *Cor Caroli*; β *Aurigæ*; *Regulus*; β , γ , δ , ϵ , ζ , and η *Leonis*.

[To be continued.]

LXII. On a Problem in the Calculus of Variations.

By I. TODHUNTER, M.A., F.R.S.*

THE problem to which the following remarks relate may be thus enunciated:—To determine the greatest solid of revolution, the surface of which is given, and which cuts the axis at two fixed points. The problem has been discussed in the *Philosophical Magazine* for July 1861, August 1861, September 1862, and March 1866; but something may, I think, be added to what has been already published.

Adopting the usual notation, we have to make $\pi \int y^2 dx$ a maximum, while $2\pi \int y \sqrt{1+p^2} dx$ is given. Let a be a constant at present undetermined; then we have by the received theory to make u a maximum, where u denotes

$$\int \{y^2 + 2ay \sqrt{1+p^2}\} dx.$$

We obtain

$$\delta u = \int A(\delta y - p \delta x) dx + B,$$

where A stands for

$$2y + 2a \sqrt{1+p^2} - 2 \frac{d}{dx} \frac{ayp}{\sqrt{1+p^2}},$$

and B stands for certain terms which are free from the integral sign.

The expressions for u and δu are both supposed to be taken between limits which correspond to the fixed points on the axis.

Now by the known principles of the subject we put $A=0$; this leads in the usual way to

$$\frac{2ay}{\sqrt{1+p^2}} = b - y^2,$$

* Communicated by the Author.

where b is another constant, which is introduced by the integration.

Since the curve is to meet the axis, we have $y=0$ at certain points; hence $b=0$, and the equation becomes

$$\left\{ \frac{2a}{\sqrt{1+p^2}} + y \right\} y = 0.$$

Thus we have either $\frac{2a}{\sqrt{1+p^2}} + y = 0$, or $y = 0$. The first corresponds to a sphere, and the second to a cylinder, of indefinitely small radius. This solution is due to the Astronomer Royal; it is given in the Philosophical Magazine for July 1861, and illustrated by figures.

There is, however, one difficulty which still exists. On proceeding to verify the solution, we find that the terms denoted by B vanish at the limits; also A vanishes for all that part of the solution which corresponds to the sphere; but A does *not* vanish for the remaining part of the solution. In fact, if we put $y=0$, we find that $A=2a$. Thus instead of having $\delta u=0$, we have

$$\begin{aligned} \delta u &= 2a \int (\delta y - p \delta x) dx \\ &= 2a \int \delta y dx, \end{aligned}$$

since $p=0$ when $y=0$. The integration is to extend throughout the range which corresponds to the cylindrical portion of the solution.

It appears to me that the following is the explanation of the difficulty. It is seen on examination that a is a negative quantity. Also by the nature of the problem, corresponding to $y=0$, the value of δy must be *positive*. Thus δu , instead of being zero, is essentially a small *negative* quantity; this, however, ensures that u is a maximum, and therefore we obtain all that is required.

The following consideration will, I think, show that it is in vain to seek for any other solution instead of that proposed by the Astronomer Royal. It is an admitted result that among all figures of given surface the sphere is that which has the greatest volume. Now, in the solution of the problem which we are considering, we obtain a figure the surface and volume of which *differ infinitesimally* from those of a sphere; and hence we conclude that the volume we obtain is greater than that of any proposed figure which differs to a finite extent from a sphere with the given surface.

It should be observed that there are two forms in which the problem may be proposed: we may suppose that the solid is restricted to lie between two planes which are at right angles to

the axis of revolution and which pass through the fixed points; or we may suppose that this restriction is not imposed. We have hitherto considered the problem without the restriction; it is easy to see that the investigation will require only a slight modification if the solid should stretch beyond the space bounded by the two fixed planes just mentioned.

If, however, the restriction be imposed, the solution already obtained holds so long as the given surface is not greater than that of a sphere described on the given length of axis as a diameter; when the given surface is greater than this, the solution is, I believe, that which was proposed in the 'History of the Calculus of Variations,' page 410, and adopted in the Philosophical Magazine for August 1861 and September 1862.

In conclusion I will state briefly the grounds on which I consider that the results published in the Philosophical Magazine for March 1866 are unsatisfactory. In the first place, the solutions there obtained do not satisfy the fundamental condition $A=0$; and in the second place, they are liable to the objection which I have drawn from the admitted result respecting a sphere.

Cambridge, May 9, 1866.

LXIII. On the Heat of the Electric Spark.

By Dr. A. PAALZOW*.

TO test an electrical exploding-apparatus, the fuse of which is to be ignited by *one* spark, we must possess some knowledge of the heat of the electric spark itself. Investigations have already been made on this subject; the earlier results will be found collected in Riess's work†, whilst later researches on the question have been communicated by Poggendorff‡ and Reitlinger§. Their experiments have shown that when a series of sparks from the electrical machine or the induction-coil is discharged between wood and wood, or wood and metal, a considerable rise of temperature is the result. The production of heat, however, by the single sparks of a battery of Leyden jars could not be proved. Neither a mercurial thermometer nor a thermopile with galvanometer, when in the immediate neighbourhood of the discharge, was in the slightest degree affected. When, however, the platinum wire of Riess's air-thermometer

* Translated from the *Monatsbericht* of the Academy of Sciences of Berlin, November 1865, p. 563.

† *Lehre von der Reibungselektricität*, §§ 550 and 700.

‡ *Berliner Monatsberichte* (1861) pp. 349-377; *Pogg. Ann.* vol. xciv. pp. 310 and 632-637; vol. cxxi. p. 307.

§ *Zeitschrift für Math. und Phys.* vol. viii. pp. 146-149.

was replaced by two electrodes and sparks were allowed to leap across the intermediate space, Knochenhauer observed a depression of the column of liquid, dependent on the quantity and tension of the electricity; but he ascribed this phenomenon not to the heat generated by the sparks, but to the mechanical movement of the air.

The main object of the subjoined experiments was to establish a connexion between the heat of the spark, the quantity and tension of the electricity, and the resistance of the apparatus; and as these three magnitudes can be most readily varied and increased in the case of the Leyden jar, it seemed expedient, first of all, to study the thermal relations of sparks from the Leyden battery.

The heat of the spark was determined by three different methods:—

(a) By means of a thermopile and galvanometer. For this purpose one face of the pile was provided with a cap of vulcanite fitting nearly air-tight. Into this cover brass electrodes were screwed (air-tight) from 4 to 5 millims. apart. They were so situated with regard to the pile, that sparks could not leap from them on to its face. When a discharge took place between the electrodes, the warmed metal and the particles of air affected the pile both by radiation and conduction.

(b) By the use of Riess's air-thermometer.

(c) By means of a finely-graduated mercurial thermometer.

The following results were obtained:—

The heat of the spark increases with the quantity and tension of the electricity. With the necessary amount of resistance (by which is understood that of the short thick copper wires connecting the electrodes with the outer and inner coats of the battery) it has its greatest value. If the resistance be increased, the heat decreases and reaches a minimum. Let the resistance be made still greater, and the heat again increases, attaining a second but smaller maximum. By further augmenting the resistance, it again declines till it becomes *nil*, in which case the resistance is so great that the battery can no longer discharge itself,—a condition attained by the introduction of a column of water of requisite length into the circuit.

Those degrees of resistance which lie near the point at which the heat of the sparks attains its second maximum are characterized by the following phenomena:—

- (1) The striation of light in rarefied gases.
- (2) The incandescence of the negative wire when fine wires are employed as electrodes.
- (3) The absence of Abria's lines in fine powders,—a proof that the air can scarcely be set in motion mechanically.

It appears therefore that intense heating of the sparks by great resistance in the circuit, striation of light, and incandescence of the negative wire are only observed when the duration of discharge has been augmented by great resistance, and when the velocity of electricity, and consequently the mechanical movement of the medium in which the spark appears is very small—in which case only could we first predicate of the air that it plays the part of conductor.

Were it desired to establish a connexion between these results of the dependence of the heat of the spark on resistance, and the known laws of the rise in temperature of metals and liquids by means of electric currents, we should have to assume, in the case of the sparks between the first maximum and first minimum, that the *incandescent metallic particles* which are projected into the air along the course of the spark conduct almost exclusively and become as much heated as if they constituted, as regards resistance, a measurable part of the entire circuit. It follows from this, in the case of the sparks near the second maximum, on the other hand, that the *air* is almost exclusively the conductor, and its resistance is very great in comparison with the whole of the remainder of the circuit, no matter whether this consist of solid or liquid bodies.

The sparks near the second maximum presenting a great similarity to the electric brush, those of the electrical machine were also examined in respect to their heating-powers. It was found that “brush” sparks with hissing noise develope much more heat than those of any other kind. The *form* of the electrode has consequently an influence on the heat of the spark, the brush being formed with less or more difficulty according as the electrodes are pointed or rounded.

The results which this inquiry into the heat of the electric spark has furnished are therefore well worth consideration in the construction of electrical exploding-apparatus. For it has been shown by experiments on another occasion that fuses divide themselves into two classes—those which can be fired by any kind of electric spark, and those the ignition of which *only* takes place by sparks near the second maximum. If therefore a fuse of the latter class be chosen, such a degree of resistance must be given to its exploding-apparatus that sparks of this kind are produced.

LXIV. *On the Fluid Theory of the Earth.*

By Archdeacon PRATT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the third edition of my 'Treatise on the Figure of the Earth' I have introduced the following proposition (see page 80, Art. 84):—

"PROP. *To prove that if the form of the earth's surface be a spheroid of equilibrium, the earth's mass must necessarily be arranged according to the fluid law, whether the mass is or has been fluid or not, in part or in whole.*"

Exception has been taken by a friendly reviewer of the book in your Number for February last, to my demonstration of this proposition; and I am free to admit that his criticism has some truth in it in the abstract. I think, however, as he has left the matter, it stands in a wrong light; and I feel no doubt that you will not grudge some of your space being taken up in discussing a point which is of very great importance.

The step in my reasoning which he objects to is this:—"Suppose some change were made in the arrangement of the earth's mass without altering its external form. It is evident that, although the resultant attraction of the whole mass on the surface might possibly be unaltered by this change at particular points of the surface, it could not remain the same as before at every point of the surface." From the well-known property of a spherical shell—that it attracts an external point precisely as if the mass of the shell were concentrated into its centre—he shows that if equal portions of the mass at the same distance on all sides around any point in its interior were moved to equal distances towards or from that point, so as still in themselves to form a complete spherical shell, the attraction of the whole on any external point would remain unaffected by the change. This exception had occurred to me before the proofsheets left me. But it seemed to imply an arrangement of the mass so incongruous that I dismissed the thought—a thought which was neither suggested nor supported by any physical considerations, but is solely a creation of the integral calculus. I see that I dismissed it too hastily; for in what I have written there are a few expressions which are stronger than I should have used, *e. g.* "must necessarily" and "could not" in the above quotations; and "the only possible law" at the end of the article. When the book issued from the press, I regretted that I had not mentioned this exception, for the purpose of replying to it, as I do below. It is a singular coincidence that I began a letter to you

on this subject about three months before the appearance of your notice of my book in February last; but the latter remains uncompleted, as I have not been able to satisfy myself on an investigation upon which I entered to determine whether there were any other exceptions than that presented by the sphere.

2. Having said thus much, I will now discuss the proposition. The object I had in introducing it I state in the previous article of my book. It was to refute a wrong notion which has of late years appeared in print, that the law of gravity at the surface of the earth can be obtained theoretically without any reference to the arrangement of the mass. The original investigation from which this result was supposed to flow is referred to at p. 190 of Mr. Airy's 'Mathematical Tracts,' fourth edition; and the following is an extract from the investigation itself:—

“Without assuming the earth's original fluidity, but merely supposing it consists of nearly spherical strata of equal density, and observing that its surface may be regarded as covered by a fluid, inasmuch as all observations relating to the earth's figure are reduced to the level of the sea, Laplace has established a connexion between the form of the surface and the variation of gravity, which in the particular case of an oblate spheroid agrees with the connexion which is found on the hypothesis of original fluidity. The object of the first portion of this paper is to establish this general connexion without making any hypothesis whatever respecting the distribution of matter in the interior of the earth, but merely assuming the theory of universal gravitation.”

In a note at p. 83 of my book I point out a false step in the reasoning of this investigation, and in art. 86 show that when this is corrected the investigation is in no respect more general, nor assumes less, than Laplace's. But I was anxious, besides showing where that particular investigation broke down, to prove *à priori* that the notion which it was attempted to establish is not true. For this purpose my proposition under consideration was produced, which, for this single object, would better have been worded as follows:—

PROP. *To prove that if the form of the earth's surface is a spheroid of equilibrium, the arrangement of the earth's mass cannot be independent of that form.*

For “suppose some [arbitrary] change were to be made in the arrangement of the earth's mass without altering its external form. It is evident that, although the resultant attraction of the whole mass might possibly be unaltered by this change at particular points of the surface, it could not remain the same as before at every point of the surface,” unless the arbitrary change happen to satisfy the precise equations of condition for the external attraction of the mass to remain the same. In the first

case the change would alter the force acting at the surface, which would therefore cease to be a surface of equilibrium; in the second case the external surface retains its form only under the condition that the arrangement of the mass is one of certain definite arrangements determined by equations of condition. In neither case, therefore, is the form of the external surface independent of the internal arrangement of the mass. To arrive at this conclusion was the object of my proposition.

3. But though I have in the last paragraph changed the wording of the proposition to meet the case I was considering, I have no intention whatever of abandoning the proposition itself in its bearing upon the question of the fluid-arrangement of the earth's mass, further than that I would change the words "must necessarily be arranged" into "is undoubtedly arranged;" for the first words would seem to exclude the mathematical possibility of any other arrangement; but the second exclude only the physical probability of a different distribution. I should be wrong to assert that it is impossible for a pyramid to stand balancing on its apex; but I should be safe in saying that it would be physically impracticable. So in the case before us, as I hope now to show, any other than the fluid-arrangement—whether or not the earth is or ever was fluid, in whole or in part—appears to me to be physically improbable in the highest degree; so that I regard the mass as being undoubtedly so arranged. My critic thinks that I have very much over-estimated the force of the evidence for the fluid-arrangement; and he considers that it does not rise above a slight probability. I am not conscious of being actuated by a spirit of retaliation; but I consider that he has very much underrated the evidence, and I am tempted to think that he has not dwelt upon it sufficiently to see its full nature. Some of his expressions have led me to this conclusion. To this I now address myself.

4. The state of the case is this:—that (1) the fluid-arrangement of the mass, whatever its past or present circumstances as regards fluidity and solidity, precisely meets the condition that the surface is a spheroid of equilibrium; (2) any general or arbitrary change of the arrangement of the mass will lead to a violation of this condition; and (3) only a class of precise changes in the distribution of the mass, limited by the condition that the external attraction shall remain unchanged by the redistribution of the mass, will leave the form of the surface unaffected.

For convenience we may represent any redistribution of the mass of the earth, in the removal of materials from one part of its volume to another, by the algebraical addition or superposition of an Imaginary Body, parts of which are of positive and other parts of negative density, the total mass being zero. In the case

before us this body is to possess the property that its external attraction is zero, and its surface is to lie wholly within the earth's surface. For brevity I shall henceforth call such a body "the Imaginary Body."

The conclusion, then, to which the above statement of the case leads is this—that the arrangement of the earth's mass must be either

- (1) The fluid-arrangement, or
- (2) The fluid-arrangement + the imaginary body.

The high physical improbability of this latter I am now to show.

5. It will be observed that it is impossible altogether to get free from the fluid-arrangement. In any arrangement of the mass which differs from it and suits the problem, still that arrangement consists of the fluid-arrangement with some other precise arrangement superadded. I will take the example which my critic produces, and will make use of his diagram (p. 149, *Phil. Mag.* for February). The distribution of the materials in this particular case is this: everywhere it follows the fluid-law, except along the surface of two spherical shells C and D; but even there the fluid-law has a controlling influence; for the distribution along those surfaces is that of the fluid-law in the case of C, *minus* a precisely equal quantity of matter from every equal elementary portion of the surface and from exactly the same depth (*i. e.* the thickness of the shell), neither more nor less from any part of the whole spherical surface all round, and in the case of D *plus* precisely the same amount of matter which is taken from C, and this to be distributed uniformly all over the surface of D and to a uniform depth all round. Such a local and, I will call it, freakish departure from the law of the rest of the mass may be mathematically possible, as it undoubtedly is; but I regard it as physically inadmissible. For this reason it was that I rejected the thought, as already related, as soon as it occurred to me. If a *vera causa* could be shown for such a singular distribution, I would at once yield the point, strange as such a distribution would be. It is, however, a mere analytical figment, which in my opinion is of no value in this problem.

But the incongruous result to which the hypothesis of this single pair of complementary shells leads may no doubt be disguised (but only disguised) by imagining the superposition of countless numbers of pairs of complementary shells, of all sizes, intersecting and crossing in endless variety, so as to reduce the mass, in the view of an imaginary eye which could survey all its parts, to a complete medley, and yet the external attraction would be unaffected and the surface-spheroid undisturbed, while the fluid-distribution was to all appearances utterly destroyed. But

that supposed distribution would be no medley and without a law; its law, however, would be so singular as to make the hypothesis on which its existence depends utterly untenable. The matter at the various points of the earth, in excess or defect of that required by the fluid-law, must be of that precise amount that by its imaginary transference by complete spherical concentration or expansion, *and by no other process*, the fluid-arrangement would be exactly attained, no matter left in excess in any point throughout the whole mass, and none in defect. My critic seems to lose sight of the extreme peculiarity of this departure from the fluid-law of distribution, when he says that "it need not be inconsistent with the ascertained variations of gravity if there were distributed irregularly through the earth very dense masses of matter, and likewise cavities filled with very light forms of matter." But his readers will probably not suspect, after reading these words, that those "dense masses" must consist of matter following the fluid-law *plus* masses lying in complete and uniformly thick and uniformly dense spherical shells, and that those "light forms of matter" also must follow the same precise law, viz. the fluid-law *minus* complete spherical shells, as before. Any departure from this singular distribution which analysis (hardly nature) has devised, would at once be fatal to the hypothesis. It may perhaps be said that these shells need not be precisely spherical; that is, they need only be nearly spherical, so as to introduce no sensible effect on pendulum experiments. But though this may appear to modify the case, it by no means removes the difficulty which the peculiarity of the arrangement presents. To my own mind these hypothetical departures from the fluid-distribution, which are possible according to the integral calculus, cannot be entertained for one moment in the face of the physical curiosities they would introduce.

Should there be any other varieties of the imaginary body besides spherical shells, the same remarks will apply.

6. Let me conclude by making some remarks on my reviewer's observation, "We do not see the need of having any theory at all." If this refers to any theory of the earth's origin, I do not see any reason to object to his remark; because it must be a matter of speculation, so far as science can feel its way. But if he refers to the theory of the earth's having been once in a fluid state, I would say (1) that if the earth's mass is now distributed in a manner which coincides with the arrangement it would have were it a fluid mass, it is so very easy and innocent an induction to pass from the one to the other, without incurring the charge of wild speculation, that it seems allowable; and (2) that not a single step has been made, that I am aware of, towards determining the figure of the earth (beyond its being merely a more or

less spherical body) without the help of the fluid-theory. Newton's first notions regarding the oblate form of the earth were derived from reflections on fluid-pressure. The geodesist measures the lengths and amplitudes of his arcs, and, *assuming* that they are of an elliptic form, determines on that hypothesis the mean axes. But whence has he derived his conception that the form of the earth may be an oblate spheroid? From the fluid theory, and nowhere else. We cannot do without the fluid theory; and to my own mind the evidence, especially from pendulum experiments, and more particularly the influence of the bulging out of the internal strata upon the pendulum (see from p. 85 to 97 of my Treatise), is quite overwhelming in favour of the conclusion that the distribution of the mass at present is proximately according to the fluid-law.

J. H. PRATT.

Calcutta, March 24, 1866.

LXV. *On Calorescence.*By Professor J. TYNDALL, LL.D. *Camb., F.R.S. &c.*

[With a Plate.]

[Concluded from p. 396.]

§ 4. **I**N the combination of bisulphide of carbon and iodine we find a means of filtering the composite radiation from any luminous source. The solvent is practically transparent, while the dissolved iodine cuts off every visible ray, its absorptive power ceasing with extraordinary suddenness at the extreme red of the spectrum. Doubtless the absorption extends a little way beyond the red, and with a very great thickness of solution the absorption of the extra-red rays might become very sensible. But the solution may be employed in layers which, while competent to intercept every trace of light, allow the invisible calorific rays to pass with scarcely sensible diminution.

The *ray-filter* here described was first publicly employed in the early part of 1862*. Concentrating by large glass lenses the radiation of the electric lamp, I cut off the visible portion of the radiation by the solution of iodine, and thus formed invisible foci of an intensity at that time unparalleled. In the autumn of 1864 similar experiments were executed with rock-salt lenses and with mirrors. The paper "On Luminous and Obscure Radiation," already referred to, contains an account of various effects of combustion and fusion which were then obtained with the invisible rays of the electric light and of the sun†.

* Philosophical Transactions, 1862, p. 67, note.

† To the experiments there described the following may be added, as

From the setting of paper on fire, and the fusion of non-refractory metals, to the rendering of refractory bodies incandescent, the step was immediate. To avoid waste by conduction, it was necessary to employ the metals in plates as thin as possible. A few preliminary experiments with platinum-foil, which resulted in failure, raised the question whether, even with the *total radiation* of the electric light, it would be possible to obtain incandescence without combustion. Abandoning the use of lenses altogether, I caused a thin leaf of platinum to approach the ignited coal-points. It was observed by myself from behind, while my assistant stood beside the lamp, and, looking through a dark glass, observed the distance between the platinum-foil and the electric light. At half an inch from the carbon-points the metal became red-hot. The problem now was to obtain at a greater distance a focus which should possess a heating-power equal to that of the direct rays at a distance of half an inch.

In the first attempt the direct rays were utilized as much as possible. A piece of platinum-foil was placed at a distance of an inch from the carbon-points, there receiving the direct radiation. The rays emitted *backwards* from the points were at the same time converged by a small mirror upon the foil, and were found more than sufficient to compensate for the diminution of intensity due to the withdrawal of the foil to the distance of an inch. By the same method incandescence was subsequently obtained when the foil was removed two, and even three, inches from the carbon-points.

The last-mentioned distance allowed me to introduce between

made at the time. A glass globe, $3\frac{3}{4}$ inches in diameter, was filled with the opaque solution, and placed in front of the electric light. An intense focus of invisible rays was formed immediately beyond the globe. Black paper held in this focus was pierced, a burning ring being produced. A second spherical flask, 9 inches in diameter, was filled with the solution and employed as a lens. The effects, however, were less powerful than those obtained with the smaller flask.

Two plano-convex lenses of rock-salt, 3 inches in diameter, were placed with their flat surfaces opposite, but separated from each other by a brass ring $\frac{3}{8}$ ths of an inch thick. The space between the plates was filled with the solution, and thus an opaque lens was formed. Paper was fired by this lens. In none of these cases, however, could the paper be caused to blaze. Hollow plano-convex lenses filled with the solution were not effective, the focal length of those at my disposal being too great.

Mr. Mayall was so extremely obliging as to transfer his great photographic camera from Brighton to London, for the purpose of enabling me to operate with the fine glass lens, 20 inches in diameter, which belonged to it: the result was not successful. It will, however, be subsequently shown that both the hollow lens and the glass lens are effective when, instead of the divergent rays of the electric lamp, we employ the parallel rays of the sun.

the focus and the source of rays a cell containing the solution of iodine. The transmitted obscure rays were found of sufficient power to inflame paper, or to raise platinum-foil to incandescence.

These experiments, however, were not unattended with danger. The bisulphide of carbon is an extremely inflammable substance; and on the 2nd of November, while employing a very powerful battery and intensely heated carbon-points, the solution took fire, and instantly enveloped the electric lamp and all its appurtenances in flame. The precaution, however, had been taken of placing the entire apparatus in a flat vessel containing water, into which the flaming mass was summarily turned. The bisulphide of carbon being heavier than the water, sank to the bottom, so that the flames were speedily extinguished. Similar accidents occurred twice subsequently.

Such occurrences caused me to seek earnestly for a substitute for the bisulphide. Pure chloroform, though not so diathermic, transmits the obscure rays pretty copiously, and it freely dissolves iodine. In layers of the thickness employed, however, the solution was not sufficiently opaque; and in consequence of its absorptive power, but comparatively feeble effects were obtained with it. The same remark applies to the iodides of methyle and ethyle, to benzole, acetic ether, and other substances. They all dissolve iodine, but they enfeeble the results by their action on the extra-red rays.

I had special cells constructed for bromine and chloride of sulphur: neither of these substances is inflammable; but they are both intensely corrosive, and their action upon the lungs and eyes is so irritating as to render their employment impracticable. With both of these liquids powerful effects were obtained; still their diathermancy, though very high, did not come up to that of the dissolved iodine. Bichloride of carbon would be invaluable if its solvent power were equal to that of the bisulphide. It is not at all inflammable, and its own diathermancy appears to excel that of the bisulphide. But in reasonable thicknesses the iodine which it can dissolve is not sufficient to render the solution perfectly opaque. The solution forms a purple colour of indescribable beauty. Though unsuited to strict crucial experiments on dark rays, this filter may be employed with good effect in class experiments.

Thus foiled in my attempts to obtain a solvent equally good and less dangerous than the bisulphide of carbon, I sought to reduce to a minimum the danger of employing this substance. At an earlier period of the investigation I had constructed a tin camera, within which were placed both the lamp and its mirror. Through an aperture in front, $2\frac{3}{4}$ inches wide, the cone of

reflected rays issued, forming a focus outside the camera. Underneath this aperture was riveted a stage, on which the solution of iodine rested, closing the aperture and cutting off all the light. In the first experiments nothing intervened between the cell and the carbon-points; but the peril of thus exposing the bisulphide caused me to make the following improvements. First, a perfectly transparent plate of rock-salt, secured in a proper cap, was employed to close the aperture; and by it all direct communication between the solution and the incandescent carbons was cut off. The camera itself, however, became quickly heated by the intense radiation falling upon it, and the cell containing the solution was liable to be warmed both by the camera and by the luminous rays which it absorbed. The aperture above referred to was therefore surrounded by an annular space, about $2\frac{1}{2}$ inches wide and a quarter of an inch deep, through which cold water was caused to circulate. The cell containing the solution was moreover surrounded by a jacket, and the current, having completed its course round the aperture, passed round the solution. Thus the apparatus was kept cold. The neck of the cell was stopped by a closely-fitting cork; through this passed a piece of glass tubing, which, when the cell was placed upon its stage, ended at a considerable distance from the focus of the mirror. Experiments on combustion might therefore be carried on at the focus without fear of igniting the small amount of vapour which even under the improved conditions might escape from the bisulphide of carbon. The arrangement will be at once understood by reference to Plate IV. figs. 4 *a* & *b*, which show the camera, lamp, and filter both from the side and from the front. *xy* is the mirror, from which the reflected cone of rays passes, first, through the rock-salt window (unshaded), and afterwards through the iodine filter *mn*. The rays converge to the focus *k*, where they would form an invisible image of the lower carbon-point; the image of the upper would be thrown below *k*; and both images spring vividly forth when a leaf of platinized platinum is exposed at the focus. At *ss* (Plate IV. fig. 4 *a*) is shown, in section, the annular space in which the cold water circulates. Fig. 4 *b* (Plate IV.) shows the manner in which the water enters this space and passes from it to the jacket surrounding the iodine-cell *m*.

With this arrangement, and a battery of fifty cells, the following results were obtained:—

A piece of silver-leaf, fastened to a wire ring and tarnished by exposure to the fumes of sulphide of ammonium, being held in the dark focus, the film flashed out occasionally into vivid redness.

Copper-leaf tarnished in a similar manner, when placed at the focus, was raised to redness.

A piece of platinized platinum-foil was supported in an exhausted receiver, the vessel being so placed that the focus fell upon the platinum. The heat of the focus was instantly converted into light, a clearly-defined and inverted image of the points being stamped upon the metal. Fig. 5 (Plate IV.) represents the thermograph of the carbons.

Blackened paper was now substituted for the platinum in the exhausted receiver. Placed at the focus of invisible rays, the paper was instantly pierced, a cloud of smoke was poured through the opening, and fell like a cascade to the bottom of the receiver. The paper seemed to burn without incandescence. Here also a thermograph of the coal-points was stamped out. When black paper is placed at the focus, where the thermal image is well defined, it is always pierced in two points, answering to the images of the two carbons. The superior heat of the positive carbon is shown by the fact that its image first pierces the paper; it burns out a large space, and shows its peculiarly crater-like top, while the negative carbon usually pierces a small hole.

Paper reddened by the iodide of mercury had its colour discharged at the places on which the invisible image of the coal-points fell upon it.

Disks of paper reduced to carbon by different processes were raised to brilliant incandescence, both in the air and in the exhausted receiver.

In these earlier experiments I made use of apparatus which had been constructed for other purposes. The mirror, for example, was detached from a Duboscq's camera, first silvered at the back, but afterwards silvered in front. The cell employed for the iodine solution was also that which usually accompanies Duboscq's lamp, being intended by its maker for a solution of alum. Its sides are of good white glass, the width from side to side being 1.2 inch.

§ 5. A point of considerable theoretic importance was involved in these experiments. In his excellent researches on fluorescence, Professor Stokes had invariably found the refrangibility of the incident light to be *lowered*. This rule was so constant as almost to enforce the conviction that it was a law of nature. But if the rays which in the foregoing experiments raised platinum and gold and silver to a red heat were wholly extra-red, the rendering visible of the metallic films would be an instance of *raised* refrangibility.

And here I thought it desirable to make sure that no trace of visible radiation passed through the solution, and also that the invisible radiation was exclusively extra-red.

This latter condition might seem to be unnecessary, because

the calorific action of the extra-violet rays is so exceedingly feeble (in fact so immeasurably small) that, even supposing them to reach the platinum, their heating-power would be an utterly vanishing quantity. Still mechanical considerations rendered the exclusion of *all* rays, of a higher refrangibility than those generated at the focus, necessary to the rigid solution of the problem. Hence, though the iodine employed in the foregoing experiments was sufficient to cut off the light of the sun at noon, I wished to submit its opacity to a severer test. The following experiments were accordingly executed.

A piece of thick black paper, mounted on a retort-ring, was caused gradually to approach the focus of obscure rays. The position of the focus was announced by the piercing of the paper; the combustion being quenched, the retort-ring was moved slightly nearer to the lamp, so that the converged beam passed through the burnt aperture, the focus falling about half an inch beyond. A bit of blackened platinum held immediately behind the aperture was raised to redness over a considerable space. The platinum was then moved to and fro until the maximum degree of incandescence was obtained, the point where this occurred being accurately marked. A cell containing a solution of alum was then placed between the diaphragm of black paper and the iodine-cell. The alum solution diminished materially the invisible radiation, but it was without sensible influence on such visible rays as the concentrated beam contained. All stray light issuing from the crevices in the lamp had been previously cut off, the daylight also being excluded from the room. The eye was then brought on a level with the aperture and slowly approximated to it, until the point which marked the focus was reached. A singular appearance presented itself. The incandescent coke-points of the lamp were seen perfectly black, projected on a deep-red ground. When the points were moved up and down, their black images moved also. When brought into contact, a white space was seen at the extremities of the points, appearing to separate them. The points were seen erect. By careful observation the whole of the points could be seen, and even the holders which supported them. The black appearance of the incandescent portion of the points could of course only be relative; they intercepted more of the light reflected from the mirror behind than they could make good by their direct emission.

The solution of iodine, 1.2 inch in thickness, proving unequal to the severe test here applied to it, I had two other cells constructed—the one with transparent rock-salt sides, the other with glass ones. The width of the former was 2 inches, that of the latter nearly $2\frac{1}{2}$ inches. Filled with the solution of iodine, these

cells were placed in succession in front of the camera, and the concentrated beam was sent through them. Determining the focus as before, and afterwards introducing the alum-cell, the eye on being brought up to the focus received no impression of light. The alum-cell was then abandoned, and the undefended eye was caused to approach the focus: the heat was intolerable, but it seemed to affect the eyelids and not the retina. An aperture somewhat larger than the pupil being made in a metal screen, the eye was placed behind it, and brought slowly and cautiously up to the focus. The whole concentrated beam here entered the pupil; but no impression of light was produced, nor was the retina sensibly affected by the heat. The eye was then withdrawn, and a plate of platinized platinum was placed in the position occupied by the retina a moment before. It instantly rose to vivid redness*. The rays which produced this incandescence were certainly invisible ones, and the failure to obtain, with the most sensitive media and in the darkest room, the slightest evidence of fluorescence at the obscure focus, proved the invisible rays to be exclusively extra-red.

When intense effects are sought after, the problem is to collect as many of the invisible rays as possible, and to concentrate them on the smallest possible space. The nearer the mirror is to the source of rays, the more of these rays will it intercept and reflect, and the nearer the focus is to the same source, the smaller will the image be. To secure proximity both of focus and mirror, the latter must be of short focal length. If a mirror of long focal length be employed, its distance from the source of rays must be considerable to bring the focus near the source, but when placed at a distance, a great number of rays escape the mirror altogether. If, on the other hand, the mirror be too deep, spherical aberration comes into play; and though a vast quantity of rays may be collected, their convergence at the focus is imperfect. To determine the best form of mirror, I had three constructed: the first is 4.1 inches in diameter, and of 1.4 inch focal length; the second 7.9 inches in diameter, and of 3 inches focal length; the third 9 inches in diameter, with a focal length of 6 inches. Fractures caused by imperfect annealing repeatedly occurred; but at length I was so fortunate as to obtain the three mirrors, each without a flaw. The most convenient distance of the focus from the source, I find to be about 5 inches; and the position of the mirror ought to be arranged accordingly. This distance permits of the introduction of an iodine-cell of sufficient depth, while the heat at the focus is exceedingly powerful.

The isolation of the luminiferous æther from the air is stri-

* I do not recommend the repetition of these experiments.

kingly illustrated by these experiments. The air at the focus may be of a freezing temperature, while the æther possesses an amount of heat competent, if absorbed, to impart to that air the temperature of flame. An air-thermometer is unaffected where platinum is raised to a white heat. Numerous experiments will suggest themselves to every one who wishes to operate upon the invisible heat-rays. The dense volumes of smoke which rise from a blackened block of wood when it is placed in the dark focus are very striking: matches are of course at once ignited, and gunpowder instantly exploded. Dry black paper held there bursts into flame. Chips of wood are also inflamed: the dry wood of a hat-box is very suitable for this experiment. When a sheet of brown paper is placed a little beyond the focus, it is first brought to vivid incandescence over a large space; the paper then yields, and the combustion propagates itself as a burning ring round the centre of ignition. Charcoal is reduced to an ember at the focus, and disks of charred paper glow with extreme vividness. Sheet-lead and tin, if blackened, may be fused, while a thick cake of fusible metal is quickly pierced and melted. Blackened zinc-foil placed at the focus bursts into flame; and by drawing the foil slowly through the focus, its ignition may be kept up till the whole of the foil is consumed. Magnesium wire, flattened at the end and blackened, also bursts into vivid combustion. A cigar or a tobacco-pipe may of course be instantly lighted at the dark focus. The bodies experimented on may be enclosed in glass receivers; the concentrated rays will still burn them, after having crossed the glass. A small chip of wood in a jar of oxygen bursts suddenly into flame; charcoal burns, while charcoal-bark throws out suddenly showers of scintillations.

§ 6. In all these cases the body exposed to the action of the invisible rays was more or less combustible. It required to be heated more or less to initiate the attack of the atmospheric oxygen. Its vividness was in great part due to combustion, and does not furnish a conclusive proof that the refrangibility of the incident rays was elevated. This, which is the result of greatest theoretic import, is effected by exposing non-combustible bodies at the focus, or by enclosing combustible ones in a space devoid of oxygen. Both in air and *in vacuo* platinized platinum-foil has been repeatedly raised to a white heat. The same result has been obtained with a sheet of charcoal or coke suspended *in vacuo*. On looking at the white-hot platinum through a prism of bisulphide of carbon, a rich and complete spectrum was obtained. All the colours, from red to violet, glowed with extreme vividness. The waves from which these colours were primarily extracted had neither the visible nor the extra-violet rays com-

mingled with them; they were exclusively extra-red. The action of the atoms of platinum, copper, silver, and carbon upon these rays transmutes them from heat-rays into light-rays. They impinge upon the platinum at a certain rate; they return from it at a quicker rate. Their refrangibility is thus raised, the invisible being rendered visible.

To express this transmutation of heat-rays into others of higher refrangibility, I would propose the term *calorescence*. It harmonizes well with the term "fluorescence" introduced by Professor Stokes, and is also suggestive of the character of the effects to which it is applied. The phrase "transmutation of rays," introduced by Professor Challis*, covers both classes of effects.

§ 7. In the foregoing section I have described arrangements made with a view of avoiding the danger incidental to the use of so inflammable a substance as the bisulphide of carbon. I have since thought of accomplishing this end in a simpler way, and thus facilitating the repetition of the experiments. The following arrangement (Plate IV. fig. 6) may be adopted with safety.

A B C D is an outline of the camera.

xy the silvered mirror within it.

c the carbon-points of the electric light.

op the aperture in front of the camera, through which issues the beam reflected by the mirror *xy*.

Let the distance of the mirror from the carbon-points be such as to render the reflected beam slightly convergent.

Fill an ordinary glass flask with the solution of iodine, and place the flask in the path of the reflected beam at a safe distance from the lamp. The flask acts as a lens and filter at the same time, the bright rays are intercepted, and the dark ones are powerfully converged. F, Plate IV. fig. 6, represents such a flask; and at the focus formed a little beyond it combustion and calorescence may be produced.

The following results have been obtained with a series of flasks of different dimensions, at a distance of $3\frac{1}{2}$ feet from the carbon-points.

1. With a spherical flask, $6\frac{3}{4}$ inches in diameter: platinum was raised to redness at the focus, and black paper inflamed.

2. Ordinary Florence flask, $3\frac{1}{4}$ inches in diameter: platinum raised to bright redness over a large irregular space. Near the lamp, the effects obtained with this flask were very striking.

3. Small flask, 1.8 inch in diameter, not quite spherical: platinum rendered white-hot; paper immediately inflamed.

4. A still smaller flask, 1.5 inch in diameter: effects very good; about the same as the last.

* Philosophical Magazine, S. 4. vol. xii. p. 521.

5. The bulb of a pipette: effects striking, but not quite so brilliant as with the less regularly shaped small flasks.

It follows as a matter of course; that where platinum is raised to whiteness, the combustion of wood, charcoal, zinc, and magnesium may also be effected.

By the arrangement here described, platinum has been raised to redness at a distance of 22 feet from the source of the rays.

The best mirror, however, scatters the rays more or less; and by this scattering, the beam at a great distance from the lamp becomes much enfeebled. The effect in free air is intensified when the beam is caused to pass through a tube polished within, which prevents the lateral waste of radiant heat. Such a tube, placed in front of the camera, is represented at AB, fig. 7, Plate IV. The flask may be held against its end by the hand, or it may be permanently fixed there. With a battery of fifty cells, platinum may be raised to a white heat at the focus of the flask.

Again, instead of a flask filled with the opaque solution, let a glass or rock-salt lens (L, fig. 8), 2.5 inches wide, and having a focal length of 3 inches, be placed in the path of the reflected beam. The rays are converged; and at their point of convergence all the effects of calorescence and combustion may be obtained.

In this case the luminous rays are to be cut off by a cell (mn) with plane glass sides; it may be placed either before or behind the lens.

Finally, the arrangement shown in fig. 9 may be adopted. The beam reflected by the mirror within the camera is received and converged by a second mirror, $x'y'$. At the point of convergence, which may be several feet from the camera, all the effects hitherto described may be obtained. The light of the beam may be cut off at any convenient point of its course; but in ordinary cases the experiment is best made by employing the bichloride instead of the bisulphide of carbon, and placing the cell (mn) containing the opaque solution close to the camera. The moment the coal-points are ignited, explosion, combustion, or calorescence, as the case may be, occurs at the focus.

The ordinary lamp and camera of Duboscq may be employed in these experiments. With proper mirrors, which are easily procured, a series of effects which, I venture to affirm, will interest everybody who witnesses them, may with the greatest facility be obtained.

It is also manifest that, save for experiments made in darkness, the camera is not necessary. The mirrors and filter may be associated with the naked lamp.

I have sought to *fuse* platinum with the invisible rays of the electric light, but hitherto without success. In some experiments I have employed a large model of Foucault's lamp, which

permitted me to use a battery of 100 cells. In other experiments I employed two batteries, one of 100 cells and one of 70, making use of two lamps, two mirrors, and two filters, and converging the heat of both lamps in opposite directions upon the same point. When a leaf of platinum was placed at the common focus, the converged beams struck it at opposite sides, and raised it to dazzling whiteness. I am persuaded that the metal could be fused, if the platinum-black upon its surface could be retained. But this was immediately dissipated by the intense heat, and, the reflecting-power of the metal coming into play, the absorption was so much lowered that fusion was not effected. By coating the platinum with lampblack it has been brought to the verge of fusion, the incipient yielding of the mass being perfectly apparent after it had cooled. Here, however, as in the case of the platinized platinum, the absorbing substance disappears too quickly. Copper and aluminium, however, when thus treated, are speedily burnt up.

§ 8. Thus far I have dealt exclusively with the invisible radiation of the electric light; but all solid bodies raised to incandescence emit these invisible calorific rays. The denser the incandescent body, moreover, the more powerful is its obscure radiation. We possess at the Royal Institution very dense cylinders of lime for the production of the Drummond light; and when a copious oxyhydrogen-flame is projected against one of them it shines with an intense yellowish light, while the obscure radiation is exceedingly powerful. Filtering the latter from the total emission by the solution of iodine, all the effects of combustion and calorescence described in the foregoing pages may be obtained at the focus of the invisible rays. The light obtained by projecting the oxyhydrogen-flame upon compressed magnesia, after the manner of Signor Carlevaris, is whiter than that emitted by our lime; but the substance being light and spongy, its obscure radiation is surpassed by that of our more solid cylinders*.

The invisible rays of the sun have also been transmuted. A

* The discovery of fluorescence by Professor Stokes naturally excited speculation as to the possibility of a change of refrangibility in the opposite direction. Mr. Grove, I believe, made various experiments with a view to effect such a change; but very soon after the publication of Professor Stokes's memoir, Dr. Miller pointed to the lime-light itself as an instance of raised refrangibility. From its inability to penetrate glass screens, he inferred that the radiation of the oxyhydrogen-flame was almost wholly extra-red, an inference the truth of which has been since established by direct prismatic analysis. The intense light produced by the oxyhydrogen-flame when projected upon lime must, he concluded, involve a change of period from slow to quick, or, in other words, a virtual elevation of refrangibility. (*Elements of Chemistry*, 1855, p. 210.)

concave mirror, 3 feet in diameter, was mounted on the roof of the Royal School of Mines in Jermyn Street. The focus was formed in a darkened chamber, in which the platinized platinum-foil was exposed. Cutting off the visible rays by the solution of iodine, feeble but distinct incandescence was there produced by the invisible rays.

A blackened tin tube (A B, fig. 10, Plate IV.) with square cross section and open at one end, was furnished at the other with a plane mirror (xy) forming an angle of 45° with the axis of the tube. A lateral aperture (xo), about 2 inches square, was cut out in front of the mirror. Over this aperture was placed a leaf of platinized platinum. Turning the leaf towards the concave mirror, the concentrated sunbeams were permitted to fall upon it. In the glare of daylight it was quite impossible to see whether the platinum was incandescent or not; but placing the eye at B, the glow of the platinum could be seen by reflexion from the plane mirror. Incandescence was thus obtained at the focus of the large mirror, X, Y, after the removal of the visible rays by the iodine solution, *mn*.

To obtain a clearer sky, I had this mirror transferred to the garden of my friend Mr. Lubbock, near Chislehurst. The effects obtained with the total solar radiation were extraordinary. Large spaces of the platinum-leaf, and even thick foil, when exposed at the focus, disappeared as if vaporized*. The handle of a pitchfork, similarly exposed, was soon burnt quite across. Paper placed at the focus burst into flame with almost explosive suddenness. The high ratio which the visible radiation of the sun bears to the invisible was strikingly manifested in these experiments. With a *total* radiation vastly inferior, the invisible rays of the electric light, or of the lime-light, raise platinum to whiteness, while, when the visible constituents of the concentrated sunbeam were intercepted, the most that could be obtained from the dark rays of the sun was a bright-red heat. The heat of the luminous rays is so great as to render it exceedingly difficult to experiment with the solution of iodine. It boiled up incessantly, exposure for two or three seconds being sufficient to raise it to ebullition. This high ratio of the luminous to the non-luminous radiation, is doubtless to be ascribed in part to the absorption of a large portion of the latter by the aqueous vapour of the air. From it, however, may also be inferred the enormous temperature of the sun.

Converging the sun's rays with a hollow lens filled with the

* Concentrating the solar rays with a mirror 9 inches in diameter and of 6 inches focal length upon a leaf of platinized platinum, the metal was instantly pierced. Causing the focus to pass along the leaf, it was cut by the sunbeam as if a sharp instrument had been drawn along it.

solution of iodine, incandescence was obtained at the invisible focus of the lens on the roof of the Royal Institution.

Knowing the permeability of good glass to the solar rays, I requested Mr. Mayall to permit me to make a few experiments with his fine photographic lens at Brighton. Though exceedingly busy at the time, he in the kindest manner abandoned to my assistant, Mr. Barrett, the use of his apparatus for the three best hours of a bright summer's day. A red heat was obtained at the focus of the lens after the complete withdrawal of the luminous portion of the radiation.

§ 9. Black paper has been very frequently employed in the foregoing experiments, the action of the invisible rays upon it being most energetic. This suggests that the absorption of those rays is not independent of colour. A red powder is red because of the entrance and absorption of the luminous rays of higher refrangibility than the red, and the ejection of the unabsorbed red light by reflexion at the limiting surfaces of the particles of the red body. This feebleness of absorption of the red rays extends to the rays of greater length beyond the red; and the consequence is that red paper when exposed at the focus of invisible rays is scarcely charred, while black paper bursts in a moment into flame. The following Table exhibits the condition of paper of various kinds when exposed at the dark focus of an electric light of moderate intensity.

Paper.	Condition.
Glazed orange-coloured paper .	Barely charred.
„ red- „ .	Scarcely tinged; less than the orange.
„ green- „ .	Pierced with a small burning ring.
„ blue- „ .	The same as the last.
„ black- „ .	Pierced; and immediately set ablaze.
„ white- „ .	Charred; not pierced.
Thin foreign-post	Barely charred; less than the white.
Foolscap	Still less charred; about the same as the orange.
Thin white blottingpaper . .	Scarcely tinged.
„ whitey-brown „ . .	The same; a good deal of heat seems to get through these two last papers.
Ordinary brown „ . .	Pierced immediately, a beautiful burning ring expanding on all sides.
Thick brown	Pierced, not so good as the last.
Thick white sand-paper . .	Pierced with a burning ring.
Brown emery „ . . .	The same as the last.
Dead-black „ . . .	Pierced, and immediately set ablaze.

We have here an almost total absence of absorption on the part of the red paper. Even white absorbs more, and is consequently more easily charred. Rubbing the red iodide of mercury over paper, and exposing the reddened surface at the focus, a thermograph of the coal-points is obtained, which shows itself by the discharge of the colour at the place on which the invisible image falls. Expecting that this change of colour would be

immediate, I was at first surprised at the time necessary to produce it. We are here reminded of Franklin's experiments on cloths of different colours, and his conclusion that dark colours are the best absorbers. This conclusion, however, might readily be pushed too far. Franklin's colours were of a special kind, and their deportment by no means warrants a general conclusion. The invisible rays of the sun possess, according to Müller, twice the energy of the visible ones. A white substance may absorb the former, while a dark substance—dark because of its absorption of the feeblest portion of the radiation—may not do so. The white powder of alum and the dark powder of iodine, exposed to the action of a source in which the invisible rays greatly surpass the visible in calorific power, exhibit a deportment at direct variance with the popular notion that dark colours are the best absorbers.

§ 10. In conclusion, I would briefly refer to a few experiments made to determine the calorescence obtainable through glasses of various colours. In the first column of the subjoined Table the colour of the glass is given; in the second column the effect observed when a brilliant spectrum was regarded through the glass is stated; and in the third column the appearance of a leaf of platinized platinum when placed at the focus, after the converged beam had passed through the glass, is mentioned.

Colour of glass.	Prismatic examination.	Calorescence.
Dark red . . .	Red only transmitted . . .	Dull white heat.
Mean red . . .	Red only transmitted . . .	White heat.
Light red . . .	Yellow intercepted with greatest power.	Bright white.
Yellow . . .		Vivid red with bright yellow in centre.
Green . . .	Besides the green, a dull red fringe and a blue band were transmitted.	No incandescence.
Dark purple . .	Extreme blue and red transmitted.	Vivid orange.
Mean purple . .	Central portion of spectrum cut out.	Vivid orange.
Light purple . .	Dims the whole spectrum, but chiefly absorbs the green.	Vivid orange.
Dark blue . . .	Transmits the blue, a green band, and a band in the extreme red.	Red heat.
Mean blue . . .	Blue; a yellowish-green band and the extreme red transmitted.	Reddish-pink heat.
Light blue . . .	Transmits a series of bands—blue and green, a red band next orange, then a dark-red band, and finally extreme red.	Pink heat, passing into red.
Another blue glass.		Pink heat.
Black glass No. 1.	Dims all the spectrum: white light transmitted.	Barely visible red.
Black glass No. 2.	Whitish-green light transmitted	Dull red.
Black glass No. 3.	Deep-red light transmitted .	Bright red, orange in the middle.

The extremely remarkable fact here reveals itself, that when the beam of the electric lamp is sifted by certain blue glasses, the platinum at the focus glows with a distinct pink colour. Every care was taken to avoid subjective illusion here. The pink colour was also obtained at the focus of invisible rays. Withdrawing all the glasses, and filtering the beam by a solution of iodine alone, platinum was raised nearly to whiteness at the focus. On introducing the pale-blue glass between the iodine-cell and the focus, the calorescence of the platinum was greatly enfeebled—so much so, that a darkened room was necessary to bring it out in full distinctness; when seen, however, the thermograph was pink. A disk of carbonized paper being exposed at the obscure focus, rose at once to vivid whiteness when the blue glass was absent; but when present, the colour of the light emitted by the carbon was first a distinct pink; the attack of the atmospheric oxygen soon changes this colour, the combustion of the carbon extending on all sides as a white-hot circle. If subsequent experiments should confirm this result, it would follow that there is a gap in the calorescence, the atoms of the platinum vibrating in red and blue periods, and not in intermediate ones. But I wish here to say that further experiments, which I hope shortly to make, are necessary to satisfy my own mind as to the cause of this phenomenon.

The incandescent thermograph of the coal-points being obtained, a very light-red glass introduced between the opaque solution and the platinum reduced the thermograph both in size and brilliancy. A second red glass, of deeper colour, rendered the thermograph still smaller and feebler. A dark-red glass reduced it still more—the visible surface being in this case extremely minute, and the heat a dull red merely. When, instead of the coloured glass, a sheet of pure-white glass was introduced, the image of the coal-points stamped upon the platinum-foil was scarcely diminished in brilliancy. A thick piece of glass of deep ruby-red proved equally transparent; its introduction scarcely changed the vividness of the thermograph. The colouring-matter in this instance was the *element* gold, not the compound suboxide of copper employed in the other red glasses. Many specimens of gold-jelly, prepared by Mr. Faraday for his investigation of the colours of gold, though of a depth approaching to absolute blackness, showed themselves eminently transparent to the obscure heat-rays; their introduction scarcely dimmed the brilliancy of the thermograph. Hence it would appear that even the metals themselves, in certain states of aggregation, share that high diathermic power which the elementary metalloids have been found to display.

I have just said that a sheet of pure-white glass, when inter-

posed in the path of the condensed invisible beam, scarcely dimmed the brilliancy of the thermograph. The intense calorific rays of the electric light pass through such glass with freedom. We here come to a point of considerable practical importance to meteorologists. When such pure-white glass has carbon mixed with it when in a molten condition, the resulting black glass is still eminently transparent to those invisible heat-rays which constitute the greater part of the sun's radiation. I have pieces of glass, to all appearance black, which transmit 63 per cent. of the total heat of the electric light; and there is not the slightest doubt that, in thicknesses sufficient to quench entirely the light of the sun, such glass would transmit a large portion of his invisible heat-rays. This is the glass often, if not uniformly, employed in the construction of our black-bulb thermometers, under the impression that the blackening secures the entire absorption of the solar rays. This conclusion is fallacious, and the instruments are correspondingly defective. A large portion of the sun's rays pass through such black glass, impinge upon the mercury within the bulb, and are ejected by reflexion. Such rays contribute nothing to the heating of the thermometer.

When a sheet of common window-glass, apparently transparent, was placed between the iodine solution and the platinum-leaf at the focus, the thermograph was more dimmed than by the black glass last referred to. The window-glass here employed, when looked at edgewise, was green; and this experiment proves how powerfully this green colouring-matter, even in infinitesimal quantity, absorbs the invisible heat-rays. Perfect imperviousness might doubtless be secured by augmenting the quantity of green colouring-matter. It is with glass of this description that the carbon should be mixed in the construction of black-bulb thermometers; on entering such glass the solar rays would be entirely absorbed, and greater differences than those now observed would probably be found to exist between the black-bulb and the ordinary thermometer.

In conclusion, it gives me pleasure to mention the intelligence and skill displayed by my assistant, Mr. Barrett, in executing the numerous experiments committed to his care during the progress of this investigation.

LXVI. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, Ph.D., F.C.S.

[Continued from p. 315.]

FELDMANN* has investigated the roots of the *Laserpitium latifolium*, L., and has found that this plant, like many others which are botanically allied, contains a peculiar new crystallized substance to which he has assigned the name *Laserpitine*.

The finely-cut roots are exhausted by alcohol of 80 per cent. at a temperature of 60°, and of the filtered extracts as much alcohol as possible distilled off in the water-bath. On cooling, the liquid separates into two layers—a lower aqueous layer, and an upper brownish-coloured resinous one. This latter contains, along with some resin, nearly all the laserpitine; on separating it from the other and exposing it in shallow vessels, it forms a crumbly mass of crystals. By appropriate purification these are obtained quite pure, in colourless rhombic prisms without smell or taste. When they contain some brown resin they taste bitter. They are insoluble in water, but easily soluble in alcohol, ether, benzole, &c. Chloroform dissolves more than its own weight of laserpitine.

It melts at 114°; and the melted mass cools to an amorphous mass, which sooner or later passes into the crystallized condition. Amorphous laserpitine has a far lower fusing-point than crystallized, dissolves more easily in solvents, and separates again in the amorphous form. If laserpitine is heated beyond its melting-point, it volatilizes and sublimes, without decomposition, in oily drops.

Laserpitine is not altered by treatment with dilute hydrochloric acid, or by being heated in a current of hydrochloric acid gas. Heated in a closed tube with concentrated aqueous hydrochloric acid, a decomposition takes place, but without any definite results. This applies also to treatment with dilute sulphuric acid.

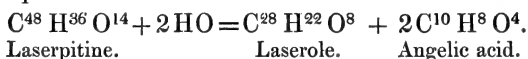
Laserpitine is decomposed by fusion with caustic potash, and also by treatment with strong alcoholic potash. To a saturated alcoholic solution of laserpitine, concentrated caustic potash was added until the precipitate at first formed was redissolved. This solution was then heated on the water-bath until all alcohol was removed. The solution was then neutralized with sulphuric acid, by which some resinous matter was separated, and the filtered solution supersaturated with dilute hydrochloric acid and distilled in a condenser as long as any acid passed over; the acid distillate was shaken with ether until the acidity was removed, and on leaving the ether to spontaneous evaporation, fine long needles

* Liebig's *Annalen*, August 1865.

were obtained, which, by their properties and by analysis, were identified as *angelic acid*, $C^{10}H^8O^4$.

While the liquid, which in the above experiment had been supersaturated with sulphuric acid, was being distilled, brownish-red oily drops were formed which gradually sank to the bottom. When these were purified, a body was obtained in a crystallized condition which had many of the properties of an alcohol, and which Feldmann calls *laserole*.

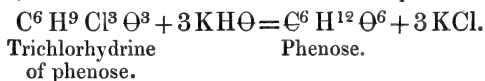
The author found the composition of laserpitine to be $C^{48}H^{36}O^{14}$; and its decomposition into laserole and angelic acid is thus expressed:



Laserpitine has thus the same quantity of carbon as athamantine and peucedanine, with which it agrees in general properties; and it is noticeable that these bodies all belong to one natural botanical family.

Some time ago Carius found* that benzole, C^6H^6 , united with hypochlorous acid to form a compound, $C^6H^9Cl^3\Theta^3$. This body he has succeeded† in transforming into a new *saccharine* substance, $C^6H^{12}\Theta^6$, which he calls *phenose*. The preparation of the body, $C^6H^9Cl^3\Theta^3$, which he calls the *trichlorhydrine of phenose*, is effected by shaking benzole with a solution of hypochlorous acid. It is very difficult to obtain in any quantity, owing to the facility with which the trichlorhydrine is oxidized by the excess of hypochlorous acid. When pure, it crystallizes in tolerably large but very thin laminæ, which under the microscope resemble benzoic acid. It melts at $+10^\circ$, and in the air attracts moisture, forming brown tarry products.

When this body is treated with caustic potash, it is decomposed, with the formation of two substances. One of them, a new acid, *benzenic acid*, which will be subsequently described, is almost exclusively formed when the action is not moderated; the other is phenose, the formation of which is thus expressed:



The action of potash is so violent, giving rise to secondary products of decomposition, that its use had to be given up. The best substitute is a dilute solution of carbonate of soda, which yields phenose almost exclusively, though by a very roundabout method, the description of which may here be omitted.

* Phil. Mag. S. 4. vol. xxvi. p. 541.

† Liebig's *Annalen*, December 1865.

Phenose is obtained as a feebly-coloured amorphous mass which deliquesces in the air; it tastes sweet like grape-sugar, with a sharp after-taste. It is soluble in water and alcohol, but not in ether. When heated it becomes brown, and above 100° it decomposes with the formation of a caramel odour.

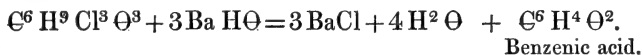
Heated either with acids or alkalies, it is rapidly changed into humus-like bodies. The latter form also an acid, which seems to correspond to the glucic acid which is formed from grape-sugar. On account of its ready changeability by alkalies, metallic compounds are very difficult to obtain. The lead-compound has the formula $\text{C}^6 \text{H}^6 \text{Pb}^3 \text{O}^6$.

Phenose is as readily oxidized as grape-sugar; the only product of the action of dilute nitric acid at a gentle heat is oxalic acid. Like grape-sugar, phenose prevents the precipitation of oxide of copper by caustic potash. Unlike grape-sugar, however, it appears incapable of fermentation.

Phenose is to be regarded as a *six-acid alcohol*, standing to benzole in a relation analogous to that which glycol bears to ethylene.

In order to obtain the acid above mentioned as a product of the decomposition of trichlorhydrine by alkalies, it is best to dissolve the trichlorhydrine in alcohol and add baryta-water in sufficient quantity, and heat on the water-bath for some hours; the excess of baryta is precipitated by carbonic acid, and the filtrate evaporated to a small volume. On the addition of hydrochloric acid to the concentrated liquid, almost the whole of the acid is separated; the filtered liquid, if shaken with ether, gives up the rest; both portions have to be purified by recrystallization.

The formation of this new acid (benzenic acid) takes place in accordance with the following equation,

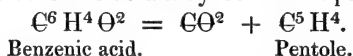


This new acid has been named benzenic acid to indicate its formation from benzole. It crystallizes in very thin colourless nacreous laminae. It melts at 110° to a colourless liquid, which distils over at 235° . It sublimes partly at 100° , and distils over with aqueous vapour, as does benzoic acid. In solution it has a strong acid reaction, and it forms well-defined salts which have great similarity to those of benzoic acid. Pentachloride of phosphorus produces with it a chloride which gives with water benzenic and hydrochloric acids.

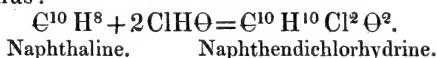
When benzenate of barium is heated with soda-lime, the benzenic acid decomposes in the same way as benzoic acid; a colourless very volatile hydrocarbon distils off without any dis-

engagement of gas, while in the residue remains carbonate mixed with unchanged benzenate and with charcoal.

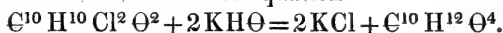
This experiment could hitherto only be made on a small scale, but it appears probable that the hydrocarbon mentioned above is a new member of the homologous series $C_n H_{2n-6}$. It has the composition $C^5 H^4$; and to express this fact, and also its connexion with the benzole series, Carius names it *Pentole*. Its formation from benzenic acid may be thus expressed:



Neuhoﬀ has described the preparation of a new tetratomic alcohol derived from naphthaline*. When this latter substance is treated with hypochlorous acid, combination ensues, with formation of a new substance which the author calls *Naphthendichlorhydrine*. Thus:

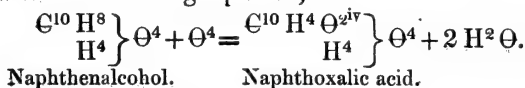


It is a clear yellow substance crystallizing in distinctly formed prisms; it melts at a gentle heat to an oily liquid, and is decomposed at a higher temperature. It is but little soluble in water, but readily so in alcohol and ether. It is easily decomposed by alkalies, in accordance with the equation



The new body here is a tetratomic alcohol, $\left. \begin{matrix} C^{10} H^8 \\ H^4 \end{matrix} \right\} \Theta^4$, and is called *naphthenic alcohol*. It is probably colourless, but rapidly becomes brown; it crystallizes in distinctly formed prisms, melts at a gentle heat, and cannot be distilled without decomposition. It is not soluble in water, but readily so in alcohol and in ether. The alcohol forms metallic compounds in which four atoms of hydrogen in its molecule are replaced by metals; these compounds, however, are difficult to obtain pure.

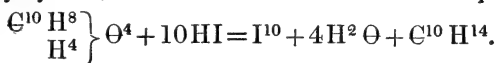
When naphthenalcohol is treated with very dilute nitric acid on the water-bath, it gradually dissolves, becoming oxidized. The clear yellow solution yields, after all nitric acid has been removed by repeated evaporation, a pale-yellow acid crystallizing in prisms. This acid the author calls *naphthoxalic acid*; it has the composition $C^{10} H^8 \Theta^6$, and stands to naphthenalcohol in the same relation as oxalic acid to glycol. It is formed in accordance with the following equation,



* Liebig's *Annalen*, December 1865.

The acid is remarkable for great stability; when heated above 100° , it sublimes in very beautiful lustrous crystals. It is a strong acid, and forms with ammonium and barium easily soluble crystallized salts.

When the acid is treated with hydriodic acid, it is decomposed with liberation of iodine, and separation of a hydrocarbon which is probably cymole, formed in accordance with the equation



Bauer has made* another addition to the series of bodies isomeric with acetylene, which he had already enriched by the discovery of rutylenet.

With a view to obtaining oxide of triamylene, he prepared the *bromide of triamylene*, $\text{C}^{15} \text{H}^{30} \text{Br}^2$, and acted with this body on acetate of silver for some days at the temperature of 100° . The mass having been exhausted with ether, the ethereal solution was evaporated and the residue distilled in an oil-bath. This distillate was treated with solid powdered caustic potash and again distilled. The product thus obtained consisted of two layers: the lower one was water; the upper layer was subjected to fractional distillation, by which it was found to consist of a liquid distilling between 230° and 240° C. On analysis this body proved to be a new hydrocarbon, which has the formula $\text{C}^{15} \text{H}^{28}$. Bauer names it *benylene*, to denote both its relation to the acetylene series and also to Walter's benic acid, to which it stands in the same relation as rutylenes to rutic acid, and acetylene to acetic acid. This new hydrocarbon stands in the same relation to triamylene as rutylenes to diamylenes, and as valerylenes to amylene. For we have

$\text{C}^5 \text{H}^{10}$ Amylene.	$\text{C}^5 \text{H}^{10} \Theta^2$ Valerianic acid.	$\text{C}^5 \text{H}^8$ Valerylene.
$\text{C}^{10} \text{H}^{20}$ Diamylene.	$\text{C}^{10} \text{H}^{20} \Theta^2$ Rutic acid.	$\text{C}^{10} \text{H}^{18}$ Rutylenes.
$\text{C}^{15} \text{H}^{30}$ Triamylene.	$\text{C}^{15} \text{H}^{30} \Theta^2$ Benic acid.	$\text{C}^{15} \text{H}^{28}$ Benylene.

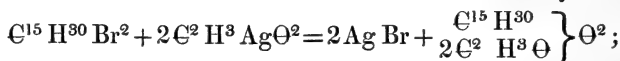
Benylene is a colourless liquid with but little odour, very viscous, and lighter than water. At low temperatures it combines with bromine without disengagement of hydrobromic acid.

Its formation may be explained by the assumption that, by the action of bromide of triamylene on acetate of silver, there are

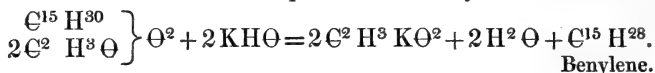
* Liebig's *Annalen*, February 1866.

† *Phil. Mag.* S. 4. vol. xxx. p. 356.

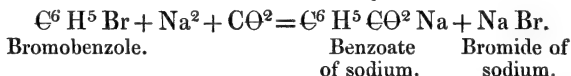
formed bromide of silver and acetate of oxide of triamylene,



and by the action of caustic potash the latter substance is decomposed into water, acetate of potash, and benylene. Thus :



Kekulé, in a paper* on the constitution of the aromatic compounds, developes with great completeness his views of their composition, and he describes a number of experiments which have been suggested by these views. Among his results is one of general interest, namely a method of obtaining from benzole and its homologues the acid corresponding to the next higher homologue: thus from benzole, $\text{C}^6 \text{H}^6$, by the addition of carbonic acid, he obtains benzoic acid, $\text{C}^7 \text{H}^6 \Theta^2$. In the case of benzole the method was as follows. Bromobenzole, $\text{C}^6 \text{H}^5 \text{Br}$, was dissolved in benzole, and sodium added in small pieces while a current of carbonic acid gas was passed through the liquid, the whole being heated in the water-bath. When the reaction was complete, the whole was dissolved in water, the oily products (consisting of benzole and unattacked bromobenzole and diphenyle, along with benzoate of phenyle and benzophenone) separated by filtration, and the solution precipitated by adding hydrochloric acid. The acid which was thus obtained had all the properties of ordinary benzoic acid. The reaction is thus expressed:—



In like manner he obtained from toluole, $\text{C}^7 \text{H}^8$, toluylic acid, $\text{C}^8 \text{H}^{10} \Theta^3$; and from xylene, $\text{C}^8 \text{H}^{10}$, xilylic acid, $\text{C}^9 \text{H}^{10} \Theta^2$.

Berthelot had shown that most organic compounds exposed to a red heat furnished acetylene. He has recently shown† that this hydrocarbon is formed under circumstances equally general: whenever an organic compound is burned in the air and produces smoke, acetylene is produced. The demonstration of this fact may be made by several interesting experiments.

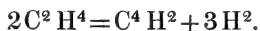
1. A gas-cylinder containing about 300 cubic centimetres is filled with olefiant gas, a few drops of ammoniacal solution of cuprous chloride introduced into it, and the gas lighted. By inclining the jar and turning it, the solution is spread out on the inside of the jar, and the flame, which at first was only

* Liebig's *Annalen*, February 1866.

† *Bulletin de la Société Chimique*, February 1866.

lighted at the mouth, gradually enters the jar. A very abundant red precipitate of cuprous acetylene is formed, both in contact with the flame and below. The experiment may be made with chloride of ethyle, C^4H^5Cl , and propylene, C^6H^6 .

With marsh-gas, C^2H , acetylene is formed in like manner, though with less brilliancy. Its formation under these circumstances is a direct proof of the condensation which marsh-gas undergoes under the influence of heat. Thus



2. It is more especially in the case of volatile liquids that this formation of acetylene in incomplete combustion is met with. It may well be shown by means of ether, a few drops of which are introduced into such a gas-jar as is mentioned above. Two or three cubic centimetres of ammoniacal solution of cuprous chloride being added and the ether lighted, the jar is inclined almost horizontally and turned round; in a moment the entire inner surface is lined with a blood-red coating of cuprous acetylene. This is a beautiful lecture experiment; and the quantity of acetylene formed makes it probable that a convenient method for preparing acetylene may be based on this experiment.

This experiment also succeeds with all neutral organic liquids which do not mix with the reagent, and volatile below 60° or 80° .

3. It may also be shown, in all cases of fuliginous flame, by placing over the flame, at such distance as not to interfere with the combustion, a funnel; by means of an aspirator, a dry empty gas-bottle is filled with the gaseous products of combustion by aspiration. In a few minutes two or three cubic centimetres of cuprous solution are poured into the flask, and the characteristic precipitate is at once formed.

This experiment may be made either with gases passed through a narrow orifice before combustion, with very volatile liquids inflamed in a capsule, or with difficultly volatile liquids heated to such a point in a capsule that they will take fire and burn continuously. Berthelot has tried in this way ordinary ether, benzole ($C^{12}H^6$), oil of turpentine ($C^{20}H^{16}$), petroleum, stearic acid, vegetable oils, and finally naphthaline.

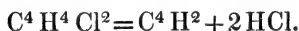
All gases and vapours of hydrocarbons are decomposed when traversed by electric sparks; and in a subsequent paper* Berthelot shows that when hydrogen is simply mixed with a gas containing carbon, and a series of sparks passed through the mixture, acetylene is formed. Thus in a few minutes a quantity of acetylene is formed in a mixture of cyanogen and hydrogen gases when traversed by the electric spark. It is also formed, and with deposition of sulphur, when the spark passes through

* *Bulletin de la Société Chimique*, March 1866.

a mixture of hydrogen and bisulphide of carbon. The action is slower, however, than in the preceding case.

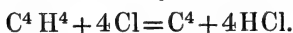
The formation of acetylene in a mixture of carbonic oxide and hydrogen is still more difficult; the passage of the electric spark must be continued for ten hours.

De Wilde describes* several experiments which he had undertaken with a view of obtaining a ready means of preparing acetylene. Dutch liquid, chloride of ethylene, $C^4H^4Cl^2$, may be regarded as consisting of acetylene and hydrochloric acid; and it might possibly, under the influence of heat, decompose into acetylene and hydrochloric acid, according to the equation



When the vapour of chloride of ethylene is passed through a porcelain tube heated to bright redness, a portion does indeed decompose in this manner, but the quantity formed is so small as to render this mode of formation of little use; in fact 100 grms. of the liquid only yielded about 2 litres of the gas.

A well-known experiment consists in mixing ethylene gas and chlorine, and setting fire to the mixture, by which a large quantity of carbon is deposited,—it being assumed that the mixture is decomposed into carbon and hydrochloric acid:

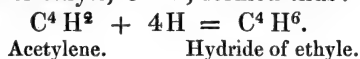


De Wilde has shown that in this case some acetylene is formed. He also found, independently of Berthelot, that acetylene is formed in the incomplete combustion of ethylene and of coal-gas, but, as Berthelot observes, did not establish the general character of this reaction.

The remarkable property which platinum-black and spongy platinum possess of condensing large quantities of gas, led De Wilde to try the experiment whether hydrogen could not in this way be fixed on certain organic compounds. An experiment was first made with acetylene. A small quantity of compressed platinum-black of the size of a pea was introduced into a measured quantity of hydrogen in a jar of this gas standing over mercury; the platinum was then removed and placed in a measured volume of acetylene. An immediate absorption is set up, which is complete after the lapse of half an hour. Numerous experiments showed that when the hydrogen was in excess, two volumes of hydrogen absorbed exactly one volume of acetylene; the odour of acetylene completely disappeared, and its presence could scarcely be detected by means of ammoniacal solution of

* *Bulletin de la Société Chimique*, March 1866.

cuprous chloride. The gas formed has no smell, it burns with a luminous flame, and is not absorbed either by fuming sulphuric acid or by bromine. It is therefore not ethylene. It is probably hydride of ethyle, $C^4 H^6$, formed thus :



LXVII. *On the Fundamental Ideas of Matter and Force in Theoretical Physics.* By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

IN an article contained in the Number of the Philosophical Magazine for May, I have completed the discussion and exemplification of the mathematical reasoning proper for determining the motion and pressure of an elastic fluid, so far at least as that reasoning may be required for the maintenance of the general physical theory respecting matter and force, the principles of which I have on several occasions expounded in this Journal. I propose now, with the aid of the mathematical results obtained in that and previous communications, to reassert those principles, and to answer certain objections to them which I have chanced to meet with in the writings of my contemporaries. But before commencing this discussion, I am desirous of adverting as briefly as possible to two points in the mathematical reasoning which have recently appeared to me to admit of further elucidation.

The first point is the numerical determination of the velocity of propagation in an elastic fluid. Assuming that the analytical expression for it has been correctly found to be $a\left(1 + \frac{e\lambda^2}{\pi^2}\right)$,

I propose to consider in what way the numerical value of the factor by which a is multiplied may be obtained. In the course of supplementary considerations relating to the Undulatory Theory of Light contained in the Philosophical Magazine for May 1865, I have shown that my first determination of the value of that factor is erroneous, and have arrived at a different result by a process the legitimacy of which I have not hitherto seen reason to question. In this new method, instead of the equation to which the mathematical reasoning conducts, viz.

$$\frac{d^2 f}{dr^2} + \frac{df}{rdr} + 4ef = 0,$$

the integral of which can only be expressed in a series, another equation,

$$\frac{d^2 f}{dr^2} + \frac{df}{rdr} + 4ef = \frac{f}{r^2},$$

* Communicated by the Author.

which can be exactly integrated, is employed, the application of its integral being restricted to large values of r , for which the two equations are clearly identical. But, as is intimated in the article above referred to, the result thus obtained is also deducible by employing only the first equation, if the point of no velocity be taken on the axis of the vibrations. The two processes are therefore confirmatory of each other; and as the latter is the more direct way of arriving at this important physical determination, I have thought proper to give it here in detail.

At the end of an article in the *Philosophical Magazine* for December 1852, reasons are given for concluding that a set of longitudinal and transverse vibrations symmetrically disposed about an axis may, for points contiguous to the axis, be resolved into two equal sets the transverse vibrations of which are at right angles to each other, and that each set may be regarded as independent of the other. (This in fact is the theoretical explanation of the polarization of light.) By reference to the same article, the equations applicable to a set of vibrations the transverse motion of which is parallel to the axis of x , will be found to be the following:—

$$\phi = m \cos q(z - \kappa at + c), \quad \kappa = \left(1 + \frac{4e}{q^2}\right)^{\frac{1}{2}}, \quad a^2\sigma + f \frac{d\phi}{dt} = 0,$$

$$f = \cos 2\sqrt{e}x, \quad u = \phi \frac{df}{dx}, \quad w = f \frac{d\phi}{dz},$$

q being put for $\frac{2\pi}{\lambda}$. Hence

$$w = -mq \cos 2\sqrt{e}x \sin q(z - \kappa at + c),$$

$$u = -2m\sqrt{e} \sin 2\sqrt{e}x \cos q(z - \kappa at + c),$$

$$a\sigma = -mq\kappa \cos 2\sqrt{e}x \sin q(z - \kappa at + c).$$

Suppose an exactly equal set of vibrations to be propagated in the contrary direction, and let w' , u' , σ' be the velocities and condensation resulting from the two sets. Then measuring z' from a point of no velocity on the axis, and substituting $\frac{2\pi}{\lambda'}$, or q' , for $2\sqrt{e}$, and κ' for $\frac{\kappa\lambda'}{\lambda}$, the following system of equations may be formed:—

$$w' = -2mq \cos q'x \sin qz' \cos q\kappa at,$$

$$a\sigma' = 2mq\kappa \cos q'x \cos qz' \sin q\kappa at,$$

$$u' = -2mq' \cos qz' \sin q'x \cos q'\kappa' at,$$

$$a\sigma' = 2mq'\kappa' \cos qz' \cos q'x \sin q'\kappa' at.$$

Hence at points contiguous to the axis the transverse velocity and condensation are expressed by formulæ exactly analogous to those which express the direct velocity and condensation, and, the direct velocity of propagation being κa , the transverse velocity of propagation is $\kappa' a$. Also, substituting $\frac{\pi^2}{\lambda'^2}$ for e in the

value of κ , we have $\kappa = \left(1 + \frac{\lambda^2}{\lambda'^2}\right)^{\frac{1}{2}} = \left(1 + \frac{\kappa^2}{\kappa'^2}\right)^{\frac{1}{2}}$; so that $\kappa'^2 = \frac{\kappa^2}{\kappa^2 - 1}$. But another relation between κ' and κ may be ob-

tained by a consideration of the *apparent elasticities* in the two directions. From the foregoing values of w and σ for a single series of waves, it follows that $w = \frac{a\sigma}{\kappa}$. If the motion had been

in parallel lines, and therefore unaffected by transverse action, the relation would have been $w = a\sigma$. Hence the transverse action *diminishes* the velocity corresponding to a *given* condensation in the ratio of $\frac{1}{\kappa}$ to 1, and consequently changes the actual elasticity a^2 into the *apparent* elasticity $\frac{a^2}{\kappa^2}$. Now this trans-

verse action may be regarded as the effect of a tendency of the direct vibrations to *lateral spreading*; in which case the direct vibrations have on the transverse vibrations just the inverse effect of the transverse on the direct, and consequently change the actual elasticity a^2 in the transverse direction into the *apparent* elasticity $\kappa^2 a^2$. The ratio of this apparent elasticity to the other is κ^4 ; and the same ratio, inferred from the squares of the velocities of propagation in the two directions, is $\frac{\kappa'^2}{\kappa^2}$, or $\frac{1}{\kappa^2 - 1}$.

Hence $\kappa^6 - \kappa^4 = 1$. The numerical value of κ is thus found to be 1.2106.

The other point which I purposed to consider is the relation between the composite velocity V and the composite condensation S when the motion is in parallel straight lines, and terms of the second order are taken into account. In the May Number I have obtained, on the supposition that $(d\psi) = udx + vdy + wdz$, a value of ψ to terms of the second order, applicable generally to vibrations relative to a *single* axis, viz.

$$\psi = -m \Sigma \cdot \left[\frac{f}{q} \cos q\zeta \right] - m^2 A \Sigma \cdot \left[\frac{g}{2q} \sin 2q\zeta \right] + m^2 Q.$$

As ζ was put for $z - \kappa at + c$, and f and g are functions of r the distance from the axis, and as the equation which determines Q proves it to be a function of $z - \kappa at$ and r , it follows that the complete value of ψ to terms of the second order is a function

of the same quantities. Hence for a given value of r , the velocity w parallel to the axis and the condensation σ are functions of $z - \kappa at$. We may therefore conclude that the composite velocity and condensation relative to an axis, so far as their expressions to terms of the second order indicate, are propagated in directions parallel to the axis with the uniform velocity κa .

In the same article the reasoning is extended to the case of vibrations relative to any number of axes having any positions in space, terms of the second order being still included; and on examining the conclusion of the investigation, it will be seen that, for the case in which the axes are all parallel, the resultant velocity parallel to the axes and the resultant condensation are at each point functions of $z - \kappa at$. Hence this composite velocity and condensation relative to any number of parallel axes, so far as terms to the second order indicate, are propagated with the uniform velocity κa . It has already been argued, when terms of the first order were alone considered, that if there were an unlimited number of sets of vibrations relative to parallel axes, and all in the same phase of vibration, the transverse vibrations would be neutralized, and the resultant motion be in parallel straight lines. And clearly this condition can be fulfilled when the motion and condensation expressed by terms of the second order are included, these being much smaller than the motion and condensation expressed by those of the first order. Now, when the motion is in parallel straight lines, and the velocity and condensation are propagated with a uniform velocity, it may be proved (see *Phil. Mag.* for September 1865, p. 218) that there exists between the velocity V , the condensation S , and the rate of propagation κa , the relation expressed by the equation

$$S = \frac{V}{\kappa a} + \frac{V^2}{\kappa^2 a^2}.$$

For composite vibratory motion, supposing the transverse motions to be neutralized, the general value of V to the first order of small quantities is $\Sigma . [m \sin q(z - \kappa at + c)]$, the number of terms being unlimited, and the coefficient m being the same for all and as small as we please. Hence, grouping the terms according to the values of q , and supposing n to be the number of terms in any one group, it may be shown, as in the *May Number* (p. 349), that the composite velocity for that group is $mn^{\frac{1}{2}} \sin q(z - \kappa at + \theta)$. Hence the whole composite velocity is

$$m \Sigma . [n^{\frac{1}{2}} \sin q(z - \kappa at + \theta)],$$

n , q , and θ being in general different for each term embraced by Σ . In order to obtain S to the second order of small quan-

ties, it is only required to substitute this expression for V in the foregoing equation, the second term of which will then become

$$\frac{m^2}{\kappa^2 a^2} \cdot (\Sigma \cdot [n^{\frac{1}{2}} \sin q(z - \kappa at + \theta)])^2.$$

This expression, which gives the means of drawing the inference which has been the main object of the preceding reasoning, is clearly equivalent to

$$\frac{m^2}{\kappa^2 a^2} \cdot \Sigma \cdot [n \sin^2 q(z - \kappa at + \theta)],$$

together with periodic terms having as much positive as negative value. These being omitted, the part which does not change sign is the quantity I have called ΔS at the end of the article in the May Number. Hence if the axes of the vibrations be supposed to diverge from a centre, the above argument proves that, since each number (n) that passes through a given area varies inversely as the square of the distance, the quantity ΔS varies according to the same law. The conclusion that the motion of translation of a small sphere acted upon by a series of undulations diverging from a centre varies inversely as the square of the distance, may thence be drawn as in the article cited.

Having now, in a series of articles (to which the foregoing discussion may be regarded as an addendum), maintained and elucidated the mathematical reasoning which I have employed to prove the propositions in hydrodynamics that are applied in my general Theory of Physics, I proceed to the main object of this communication, which is to assert again and defend the principles of that theory. Its fundamental hypotheses, which have been frequently stated in this Journal, and are repeated here for the sake of convenience, are these which follow. All matter consists of very minute atoms having no other properties than constancy of form, constancy of magnitude, and an intrinsic inertia which is always the same for matter of the same magnitude. All atoms are supposed to be spherical. No other kind of force is recognized than that of *pressure*. The resistance of the atoms, when pressed, to all change of form and magnitude constitutes a physical force which may be called *atomic reaction*. All other physical force has its origin in the pressure of a universally diffused elastic fluid medium called the æther, which pervades all space, and fills those portions of space in the interiors of visible and tangible substances that are not occupied by their proper atoms. The æther undisturbed has the same density and elastic force throughout its extent, but is susceptible of variations of density. The variations of its density are accompanied

by proportional variations of its pressure. The different kinds of physical forces are pressures of the æther acting under different circumstances, and are regulated by the modes of the mutual action of the parts of the fluid. The laws of the physical forces, and of the action of the æther on the atoms of visible and tangible substances, are the proper subjects of mathematical research, without which it is not possible to assign *reasons* for phenomena and the laws of phenomena.

The above hypotheses are, in part, coincident with those relating to the qualities of bodies contained in Regula III. prefixed to the third book of Newton's *Principia*; and all of them have been adopted in accordance with rules of philosophy laid down by Newton, viz. (1) to admit no qualities of bodies that are not cognizable and intelligible by sensation and experience; (2) to frame no *arbitrary* hypotheses. Where Newton says at the end of the *Principia*, "hypotheses non fingo," he is referring to *speculative* hypotheses not deducible from, or not supported by, phenomena, the same that at the beginning of Regula III. he calls "somnia." But there is nothing in his philosophy opposed to such universal and necessary hypotheses as are the foundations of *Theory*, without which, in fact, theory, regarded as the perception of reasons for phenomena, does not exist. The qualities of bodies enumerated in Regula III., such as extension, hardness, mobility, and *vis inertiae*, are *hypotheses* in the proper sense of the word, inasmuch as they are appropriate foundations of physical theory. It is certainly a remarkable circumstance that Newton has placed at the beginning of his third book, which is mainly devoted to the calculation of the movements of masses acted upon by gravity, a statement of the essential qualities of the ultimate constituents of matter, accompanied by the assertion that our perception of these qualities by the *senses* is "the foundation of all philosophy." Probably his chief motive in doing this was to distinguish between essential qualities and the attraction of gravity, respecting which he at the same time makes the assertion that he by no means affirms it to be, like *vis inertiae*, essential to bodies, assigning for the distinction the obvious reason that gravity diminishes with distance from the attracting body. In this view Newton has not been followed by modern philosophers.

Hypotheses that are made the foundations of a physical theory ought, in the first place, to admit of being expressed in terms which personal sensation and experience render perfectly intelligible; secondly, they should be such only as are suggested by observation and experiment; and thirdly, they must be proper for forming the basis of *mathematical reasoning*. This last condition is a necessary one, because the evidence for the truth of

the hypotheses, after all, rests on a comparison of consequences mathematically deduced from them with actual phenomena, and on their capability of explaining the phenomena. For the sake of illustration I shall first show how the hypotheses of the Theory of Gravitation satisfy the above conditions, before I proceed to consider whether they are satisfied by the hypotheses of my Physical Theory.

In the Theory of Gravitation it is supposed that all bodies attract each other with forces which at a given distance are proportional to their masses, and at different distances vary inversely as the squares of the distances. This law of the variation of gravity, although it might have been suggested by calculating, from data furnished by observation, as Newton did, the deflection of the moon from a tangent to her orbit, is still strictly an hypothesis, on which the theoretical calculation of the motions and paths of bodies acted upon by gravity is founded. Another hypothesis respecting the force of gravity is, that whatever it may be intrinsically, it is the same thing in kind as the force which draws towards the earth a terrestrial body free to obey its attraction. Now, with respect to this effect of the earth's attraction, observation and experiment have established the law that whether the motion be rectilinear or curvilinear, the increments of velocity estimated in the direction of the action of the force are proportional to the increments of time, whatever be the amount and direction of the actual velocity. This experimental law of accelerative force is a fundamental hypothesis of the Theory of Gravitation. The experiments of Galileo were necessary antecedents of the calculations of Newton. It is true that this law is established experimentally only for a constant force, whilst the forces in the Theory of Gravitation are variable functions of time and space. But the new order of calculation which Newton discovered, which, whatever be the mode of representing it, amounts to the formation and solution of a differential equation, gave the means of passing from the case of the constant to that of the variable force, after furnishing a symbolic expression for the latter, founded on the axiom that during an indefinitely short time a variable force acts as if it were a constant force. There is still another hypothesis of the Theory of Gravitation, viz. that the attractions of several bodies on the same body at the same instant take effect without mutual interference. This law, which might be suggested by the observed movements of the moon under the simultaneous influences of the earth and the sun, is proved to be exactly true by comparisons of the results of calculations founded upon it with results deduced from observation. The above are the only necessary hypotheses of the Theory of Gravitation; and perhaps the fore-

going statement of them with the accompanying remarks may suffice to show that they are such as to satisfy the conditions which, as before said, are requisites of the hypotheses of a physical theory. The opinion generally entertained that gravity is an essential property of matter forms no part of the theory. It is a gratuitous speculation without physical significance.

Before dismissing the subject of gravity, another remark may be appropriately made, bearing upon the relation of the force of gravity to a general physical theory. Assuming, as the present state of physical astronomy allows us to do, that the Theory of Gravitation has been established by the combination of calculation with observation, it will follow that the hypotheses of the theory are proved to be physical realities. But the terms in which they are expressed sufficiently show that they are not ultimate facts, but rather *laws*, and as such deducible by mathematical reasoning from the fundamental ideas of matter and force, it being a rule of philosophy that all *quantitative* laws admit of *à priori* deduction from initial principles. The hypotheses of gravitation being proved to be true, we might reasonably ask the questions, Why is the attracting power of a mass proportional to the mass? Why does it vary inversely as the square of the distance from the mass? What is the reason that the attraction of gravity adds in every small given interval of time an increment of velocity in the direction in which it acts proportional to the attracting power, independently of the size, density, and actual motion of the attracted body? Why is there no interference of different attractions acting at the same time on the same body? and how is it that the attractive power penetrates into the substances of bodies and passes through them with no perceptible change as to law or amount? These are all questions to which answers may be expected to be given in the progress of physical science; and it would be the special province of a general physical theory founded on the fundamental ideas of matter and force to answer by the aid of mathematics as well these questions relating to the force of gravity, as similar questions relating to the other physical forces. This brings me to the consideration of the hypotheses of my general Physical Theory already enunciated, which I now proceed to discuss one by one, with the view of showing that they are framed in accordance with the three conditions which the foundations of an intelligible physical theory are required to satisfy.

(1) Matter is not infinitely divisible like space; for then it would not be distinguishable from space. All bodies, therefore, have parts that are not separable into other parts; that is, they have ultimate parts that may be named *atoms*. The hypothesis of atomic constitution is supported by chemical analysis and

synthesis, and by the facts that bodies may be broken, may change as to form by alteration of the relative positions of the parts, and may change as to magnitude by compression or dilatation. The last fact implies that the atoms are separated by spatial intervals.

(2) The essential qualities of matter are those we perceive by the *senses*. This is Newton's fundamental principle. All bodies of which we take cognizance by the senses have form. Therefore the atoms, or ultimate constituents of bodies, have form; otherwise they want an essential quality of matter. Change of form, which we perceive bodies to be capable of, is a consequence of atomic constitution, and therefore cannot be predicated of the atoms themselves.

(3) All that is said in paragraph (2) is equally true if *magnitude* be substituted for form.

(4) We know what inertia is by experience of it in masses, and it is found by experiment to be always the same in the same quantity of matter independently of all other properties. Hence inertia is an essential quality of matter, *sui generis*, existing in masses only because it exists in their ultimate parts. Thus the atoms of bodies are essentially inert.

(5) In order to reason mathematically on the ultimate parts of bodies, it is necessary to ascribe to them a *particular* form. The *spherical* form is suggested by observation and experiment, it being found that however a given mass may be turned about, the direction of the earth's attraction remaining the same, however the different parts may change their relative positions, whether the mass be in the solid, fluid, or gaseous state, and whatever analysis or synthesis it may undergo, the action of the earth's gravity upon the whole, and therefore upon each atom (that is, the *weight* of the whole and of each atom), are constantly the same. These facts point to a spherical form as sufficiently accounting for the indifference of the atom as to the direction of the external action, and as to its being exerted under different physical circumstances. This form may therefore be taken hypothetically as the basis of mathematical reasoning applied to atoms.

(6) There is no other kind of force than *pressure* by contact of one body with another. This hypothesis is made on the principle of admitting no fundamental ideas that are not referable to sensation and experience. It is true that we *see* bodies obeying the influence of an external force, as when a body descends towards the earth by the action of gravity; and so far as the sense of sight informs us, we do not in such cases perceive either the contact or the pressure of another body. But we have also the *sense of touch* and of *pressure*, by contact, for instance, of the

hand with another body, and we feel in ourselves the *power* of causing motion by such pressure. The consciousness of this power and the sense of touch give a distinct idea, such as all the world understands and acts upon, as to how a body may be moved; and the rule of philosophy which makes personal sensation and experience the basis of scientific knowledge, as they are the basis of the knowledge that regulates the common transactions of life, forbids recognizing any other mode of moving a body than this. When, therefore, a body is caused to move without *apparent* contact and pressure of another body, it must still be concluded that the pressing body, although invisible, exists, unless we are prepared to admit that there are physical operations which are and ever will be incomprehensible by us. This admission is incompatible with the principles of the philosophy I am advocating, which assume that the information of the senses is adequate, with the aid of mathematical reasoning, to explain phenomena of all kinds. Probably it will be urged, in opposition to these views, that in cases of personal sensation of pressure, as when the hand presses against any substance, no parts of the two substances are really in contact, the molecular forces keeping the atoms asunder. I allow this to be the case, and still maintain that the idea of pressure by contact is distinctly conveyed by sensational experience, and that this idea, united with the consciousness of being *able* to move a body by pressure, is our only means of understanding cause of motion. Consequently pressure by contact, even though it be an abstraction, is the appropriate basis of scientific research respecting the action and the laws of forces.

(7) From the hypothesis that the atoms of bodies are spheres of constant magnitude, it follows that they have the property of *resisting* any pressure tending to change their form or magnitude. The pressure being supposed to be caused by the contact of an elastic fluid medium, the resistance acts as a pressure on the medium just equal and opposite to that of the medium on the atom. This power of resistance is to be regarded as an essential quality of the atom, implied in constancy of form and magnitude, and, like inertia, an ultimate fact, for which no antecedent cause is assignable. It acts, however, in a manner which is perfectly intelligible from common experience; and as its action takes place only when brought into play by the pressure of an external substance, I propose to call it *atomic reaction*.

(8) All physical force being pressure, there must be a medium by which the pressure is exerted. The antecedents of physical science have in various ways led to the hypothesis of an *ether*. The idea of this medium was fully entertained by

Newton, who has Queries about it at the end of his 'Optics,' and mentions it expressly in the very last paragraph of the *Principia*. The physical fact which perhaps more than any other suggests this hypothesis, is the ascertained velocity of light, which is so great as to be scarcely compatible with the transmission of matter, and may reasonably be ascribed to transmission of motion, like the propagation through the air of waves which produce sound. The æther is accordingly assumed to be a very elastic fluid, universally diffused, and filling all space not occupied by the atoms of visible and tangible bodies. It must be a *fluid* medium, and extremely rare, because it allows the large masses of the planets to move through its substance without perceptible impediment. As the æther is the supposed medium by which all physical force is exerted, it cannot be acted upon by any of the different kinds of physical force, as gravity, heat, magnetism, &c.; and consequently in its undisturbed state its pressure and density are everywhere the same. The fact of transmission of waves of the æther, like waves of air, implies that it is susceptible of variations of density and pressure; and these variations may be supposed to have been originally produced by mutual action between it and the solid atoms. Respecting the relation between the density and the pressure, I make the hypothesis, suggested by what is known of air of given temperature, that the pressure is always exactly proportional to the density. Here it might be objected that this is not an allowable hypothesis, because it is the expression of a quantitative law, and ought therefore, according to a rule of philosophy before adduced, to be derivable by mathematical reasoning from antecedent premises. My answer to this objection is, that on the hypothesis of the dynamical action of an æther, the law, so far as it regards air of given temperature, is, in fact, deducible by *à priori* reasoning, such as that which I have employed in an investigation relative to this law contained in the Philosophical Magazine for June 1859. Hence, in assuming that the law holds good for the æther, we attribute to the æther a quality which is suggested and made intelligible by reasoning as well as experiment, and the hypothesis is on that ground proper for forming, with others, the foundation of a general physical theory. It is consequently not necessary to inquire what may be the reason that the æther conforms to this particular law; but if the inquiry should be insisted upon, it is obvious to reply that we can conceive of the existence of another order of æther having the same relation to the first as that has to the air, and so on *ad libitum*; for "nature knows no limit." Possibly, in accounting for the law in this way, we reach the ultimate conception that we can form of phy-

sical force, or perhaps resolve it into volition. This, however, is approaching very closely to the region of metaphysics; and as far as regards *physical* research, it suffices to take the law of the proportionate variation of the density and pressure of the æther as an ultimate fact.

All the different kinds of physical force detected by observation and experiment are modifications of pressures of the æther. This is not a distinct hypothesis, but only a necessary sequel of the preceding hypotheses. Here it is proper to remark that although all the hypotheses may have been expressed in intelligible terms, and are such as observation and experience suggest, and are also a proper basis for mathematical reasoning, they are not therefore necessarily true. Their truth can only be proved by a satisfactory comparison of the mathematical deductions from them with facts. But inasmuch as they satisfy those three essential conditions of hypotheses, this circumstance exempts them from *préjudgment*, and ought to secure an impartial consideration on the part of my contemporaries of the explanations of numerous phenomena which I have derived from their mathematical consequences. Further, it is to be said that hypotheses fulfilling the above-named conditions are such that if the deductions from them admit of satisfactory comparison with facts of observation and experiment, they fully explain the facts, because the explanations rest on an intelligible basis. Hypotheses not fulfilling the same conditions do not completely explain phenomena.

With respect to the comparison of mathematical results from the hypotheses with facts, I have to remark that there is an initial difficulty, relating to the appropriate mathematical reasoning, which must be overcome before the comparison can be effected. In Newton's time, as soon as the calculation proper for finding the motion of a single physical particle acted upon by accelerative forces was discovered, physical astronomy and other branches of mechanics made rapid progress. What is required at the present time is the discovery of the correct processes for calculating the motions and pressures, under given circumstances, of a congeries of physical particles forming an elastic fluid, and influencing each other's motions by mutual pressure. Till this be done, the hypothesis of the æther cannot be brought to bear on Theoretical Physics. I take this opportunity for expressing the conviction that there ought not to be among philosophers such diversity of opinion as that existing in the present day relative to the foundations and the true method of philosophy. The proper principles and the proper course were pointed out at the Newtonian epoch of science; and in adopting the foregoing hypotheses, I have only followed out the rules then laid down, with such modifications as the existing

state of science demanded. As I have said before, I say again, that I do not hold myself responsible for these hypotheses, not perceiving how any others could be framed consistently with a due regard to the antecedent and the existing state of physics, and to the essential requisites of the foundations of science. The real difficulty that stands in the way of the progress of theoretical physics is a mathematical one—that of discovering the true methods of applying differential equations of three or more variables, and their integrals, for finding the motion and pressure of a fluid under given circumstances. This is purely a matter of *reasoning*, and by reasoning only can it be settled. It is in endeavours to surmount this difficulty that I have for many years given attention to the mathematical theory of the motion of fluids; and though I have succeeded to a considerable extent, I am well aware that much remains to be done. In particular it may be mentioned that I have not obtained an analytical expression for the quantity called h towards the end of the article in the May Number, the determination of which is of much importance relative to the theory of attractive and repulsive forces. Having spent a great deal of time and thought on these researches, I must now leave to others, who may think it worth their while, the task of pursuing them further. In the mean time I may appeal to the many explanations of phenomena already effected by means of my Hydrodynamical Theorems as evidence that the method of philosophy I have pursued is the true one, and that I am justified in recommending it for adoption. It would be tedious to enumerate here all the explanations to which I allude; any one desirous to become acquainted with them, and the various steps by which they were reached, will find the requisite information in communications to this Journal extending through the last twenty years. I will only advert to a few instances of explanations that are new or of special importance. In the Undulatory Theory of Light transverse vibrations are arrived at by reasoning from hydrodynamical principles; polarization is deduced (not assumed); the direction of transverse vibrations relative to the plane of polarization is ascertained; reasons are assigned for the observed differences between plane-polarized light, elliptically polarized light, and common light; the loss of half an undulation is accounted for; and theories are given of Dispersion and Double Refraction. For explaining phenomena of light, transverse vibrations, considered only to the first power of the velocity and condensation, suffice. When the analysis is extended to the squares of the velocities and condensations, it is shown that the æthereal waves are capable of causing motions of translation of the atoms, and thus producing, according to diversity of circumstances, the repulsion of heat, the attraction of molecular aggregation, and the

attraction of gravity. The other physical forces, electricity, galvanism, and magnetism, are referable, not to vibrations, but to *steady motions* of the æther; and many of the more obvious phenomena are at once explained by hydrodynamical theorems. It remains for me to meet certain objections which have been made to the views I have been maintaining, and to notice some obstacles which stand in the way of their being received.

In the *Philosophical Magazine* for July 1865, in a note at the bottom of page 64, I have remarked that the expression "finite atoms!" occurs. A note of admiration is no argument, but it expresses an opinion, and may express a prejudice. After the argument respecting atoms contained in this communication, I think it needless to do more than state a counter opinion. The author of this note of admiration, in upholding the law of "conservation of energy," has found it necessary to account for *waste* of energy; and in attempting to do this a difficulty is met with which has led to the making and making again of hypotheses destitute of all verisimilitude. This difficulty arises from the adoption of a system of philosophy that rejects the finite atom.

The Number of 'Good Words' for July 1865 contains, in p. 500, the following argument:—"Under no conception but that of a solid can an *elastic and expansible* medium be *self-contained*. If free to expand in all directions, it would require a bounding envelope of sufficient strength to resist its outward pressure. And to evade this by supposing it infinite in extent, is to solve a difficulty by words without ideas—to take refuge from it in the simple negation of that which constitutes the difficulty." And yet the hypothesis that the æther is an elastic fluid is an intelligible one, and may be made the basis of reasoning. If the reasoning explains phenomena so satisfactorily as to *prove* the truth of the hypothesis, we shall be compelled to admit the existence of an elastic fluid indefinitely extended. Suppose that we are placed by the argument under this necessity, I do not see that the case with respect to matter will then be different from that with respect to time or space, which we are equally compelled to think of as extended *ad libitum*.

In the *Proceedings of the Royal Society* (vol. xiii. p. 534), an argument, which I am unable to understand, is advanced purporting to show that the "explanation of the force of gravitation is not to be looked for in the action of a surrounding medium." As I do not find this assertion in the paper the abstract of which I quote from, there is no occasion to allude to it further.

I have reason to say that a main obstacle to the adoption of the method of philosophy I am arguing for is the favour with which the conception of an *isotropic constitution of the æther* has been received. This idea seems to have been suggested by the crystalline structure of certain substances that may be seen and

handled, which structure is with much probability ascribed to particular arrangements of their discrete atoms, and will, I think, eventually be accounted for by ascertaining the laws according to which the æther (such as I conceive it to be) acts on the individual atoms. But there is no reason from experience to conclude that crystalline structure is the property of masses that are not of comparatively limited extent. It plainly exists only in the solid state, and is therefore dependent on the molecular conditions which separate the solid from the fluid state. But these conditions have reference only to the state of aggregation of the atoms very near the confines of the substance, which clearly must be different from the state of aggregation in the interior, where a particular atom is held in its place by forces that are, at least *quam proxime*, the same in all directions, whereas the resultant action on an atom at and very near the boundary must needs be much in excess towards the interior. Now Tyndall's very instructive experiment of converting pounded ice into a continuous whole by mere pressure, proves that the superficial conditions of solidity, and the related conditions of crystalline arrangement, may be crushed out by a sufficient amount of pressure. There is therefore no evidence that in the interiors of large masses like the earth or the sun, where the pressure must be enormously great, there can be any atomic arrangement or condition distinguishing the solid from the fluid state; rather, in the case of the earth, there is evidence to the contrary. For the fact that the average superficial form of the solid parts of the earth is conformable to the ocean surface is to be accounted for by supposing the condition of solidity to affect only a comparatively thin crust, and to have very little influence on the general figure. Consequently our knowledge of the properties of large masses cannot be said to furnish reasons for attributing to the æther, of which both the extent and the pressure are vastly great, a crystalline structure. Still less reason is there for making the peculiar hypothesis of isotropic constitution, which was originally framed to account for the polarization of light. This hypothesis is one of the class which Newton declared that he abstained from making, and which for distinction may be called *speculative*. Although the words *speculation* and *theory* have properly the same signification, the former, since it has acquired an unfavourable sense, may be used to distinguish between a theory that rests only on *personal* and *arbitrary* conceptions, and a theory that is truly such, resting on ideas derived from sensational experience and common observation. Where Newton speaks of "somnia," he is probably referring to the Cartesian theory of vortices, which, although it was the arbitrary speculation of an individual, had an extraordinary hold upon the science of that time, as is shown by the circumstance that a

considerable portion of the *Principia* is devoted to its refutation. It is not a little singular that the isotropic hypothesis has the same hold upon modern physical science, with, as it seems to me, just as little reason, having nothing but arbitrary conception to rest upon, and being itself as difficult to explain as the facts for the explanation of which it was invented.

Another obstacle to the reception of the *à priori* method of philosophy I have been discussing is the practice which has been very prevalent of giving to a general law the name of a *principle*, and employing it deductively. The law of the conservation of *vis viva* is expressible by a formula, which is obtained by mathematical reasoning from established dynamical principles. It is therefore strictly a law, being reached by the only process that is proper for demonstrating laws, besides that of direct experiment. And yet the *principle* of the conservation of *vis viva* is frequently spoken of, and attempts have been made to construct upon it a general physical system. The attempts I refer to have not been successful, and have only stood in the way of a sounder philosophical method.

I have now accomplished my purpose of making a final effort to gain attention to views long entertained and often urged respecting the course that ought to be pursued in theoretical physics. There is, I conceive, at the present time no greater scientific need than to ascertain what is the true method of philosophy. It seems to be admitted on all sides that the experimental demonstration of facts and laws, although a necessary part of philosophy, does not constitute the whole of it. The efforts that are continually made, both by experimenters and theoretical calculators, to give *reasons* for the results of experiments is a kind of evidence that theory is felt to be necessary for completing scientific knowledge. But it must be confessed that the prevailing theoretical speculations present a wonderful diversity of views and hypotheses, and seem to be guided by no definite rules or principles, the faculty of imagining having a large share in framing them. In the greater number of the speculations the hypotheses which they involve are far from satisfying the three conditions I have insisted upon in this communication. The acceptance of those conditions is a necessary step in modern philosophy. I beg permission to add that, having after much consideration adopted hypotheses which satisfy the requisite conditions, and having to a very considerable extent tested their truth by the results of mathematical calculation, I have some reason to say that these hypotheses are the necessary foundations of theoretical research, and that I have indicated the true method of philosophy.

Cambridge, May 19, 1866.

LXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 399.]

February 15, 1866.—Lieut-General Sabine, President, in the Chair.

THE following communication was read:—

“Further Observations on the Spectra of some of the Nebulæ, with a Mode of determining the Brightness of these Bodies.” By William Huggins, F.R.S.

In the first part of this paper the author continues his observations on the spectra of nebulæ and clusters. The results already presented by him to the Royal Society are confirmed by his new observations, namely, that with his apparatus clusters and nebulæ give either a continuous spectrum or a spectrum consisting of one, two, or three bright lines. The positions in the spectrum of these lines are the same as those of the bright lines of the nebulæ described in his former papers.

On account of the faintness of these objects the author was not able to ascertain whether the continuous spectra which some of the nebulæ give are interrupted by dark lines in a manner similar to the spectra of the sun and fixed stars. Some of these spectra appear irregularly bright in some parts of the spectrum.

The nebulæ which follow have a spectrum of one, two, or three bright lines; in addition to which, in the case of some of them, a faint continuous spectrum was visible. These bodies are probably gaseous in constitution.

No. 2102	27 H. IV.	No. 4499	38 H. IV.
4234	5 Σ .	4827	705 H. I.
4403	17 M.	4627	192 H. I.
4572	16 H. IV.			

The following nebulæ and clusters give a continuous spectrum:—

No. 105	18 H. V.	No. 4315	14 M.
307	151 H. I.	4357	190 H. II.
575	156 H. I.	4437	11 M.
1949	81 M.	4441	47 H. I.
1950	82 M.	4473	Auw. N. 44.
3572	51 M.	4485	56 M.
2841	43 H. V.	4586	2081 h .
3474	63 M.	4625	51 H. I.
3636	3 M.	4627	192 H. I.
4058	215 H. I.	4600	15 H. V.
4159	1945 h .	4760	...	207 H. II.
4230	13 M.	4815	53 H. I.
4238	12 M.	4821	233 H. II.
4244	50 H. IV.	4879	251 H. II.
4256	10 M.	4883	212 H. II.

The second part of the paper contains an account of a mode of determining approximatively the intrinsic brightness of some of the nebulæ.

Analysis by the prisms shows that some of the nebulæ consist

of luminous gas existing in masses which are probably continuous; and the nebulae in the telescope present not points, but surfaces, in some cases, subtending a considerable angle. As long as an object remains of sensible size in the telescope it retains its original brightness, except as this may be diminished by a possible power of extinction belonging to celestial space, and by the absorptive power of the earth's atmosphere.

By means of a special apparatus the light of three nebulae was compared with the light emitted by a sperm candle, burning at the rate of 158 grs. per hour. The results are that— [candle.

The intensity of nebula, No. 4628 1 H. IV. = $\frac{1}{1508}$ th part of that of the

„ „ annular nebula in Lyra = $\frac{1}{6032}$ th „ „

„ „ Dumb-bell nebula = $\frac{1}{19604}$ th „ „

The estimation in each case refers to the brightest part of the nebula. The amounts are too small by the unknown corrections for the loss which the light has sustained in its passage through space and through the earth's atmosphere. These values have an importance in connexion with the gaseous nature of the source of the light, which the spectroscope indicates. Similar estimations made at considerable intervals of time might show whether the brightness of these bodies is undergoing increase, diminution, or a periodic variation.

The paper concludes with some observations on the measures of the diameters of some of the planetary nebulae. A very careful set of measures of 4232, 5 Σ , by the Rev. W. R. Dawes, F.R.S., is given, which makes the equatorial diameter = $15''.9$. Also measures by the author of 1414, 73 H. IV. which give its diameter in R. A. = $30''.8$.

February 22.—J. P. Gassiot, Esq., Vice-President, in the Chair.

The following communication was read:—

“Account of Experiments on the Flexural and Torsional Rigidity of a Glass Rod, leading to the Determination of the Rigidity of Glass.” By Joseph D. Everett, D.C.L., Assistant to the Professor of Mathematics in the University of Glasgow.

In these experiments a cylindrical rod of glass is subjected to a bending couple of known moment, applied near its ends. The amount of bending produced in the central portion of the rod is measured by means of two mirrors, rigidly attached to the rod at distances of several diameters from each end, which form by reflexion upon a screen two images of a fine wire placed in front of a lamp-flame. The separation or approach of these two images, which takes place on applying the bending couple, serves to determine the amount of flexure.

In like manner, when a twisting couple is applied, the separation or approach of the images serves to determine the amount of torsion.

The flexural and torsional rigidities, f and t , which are thus found by experiment, lead to the determination of Young's Modulus of Elasticity, M (or the resistance to longitudinal extension), and the absolute rigidity, n (or resistance to shearing), M being equal to f divided by the moment of inertia of a circular section of the rod

about a diameter, and n being equal to t divided by the moment of inertia of a circular section about the centre. The "resistance to compression," k , is then determined by the formula

$$\frac{1}{3k} = \frac{3}{M} - \frac{1}{n},$$

and the "ratio of the lateral contraction to longitudinal extension," ν , by the formula

$$\sigma = \frac{M}{2n} - 1.$$

The values found for the flint-glass rod experimented on were, in grammes' weight per square centimetre,

$$M = 614,330,000,$$

$$n = 244,170,000,$$

$$k = 423,010,000,$$

$$\sigma = .258.$$

The mode of experimenting is somewhat similar to that by which Kirchhoff investigated the value of σ for steel and brass; but there are several points of difference, especially this—that the portion of the glass rod, whose flexure and torsion are measured, is sufficiently distant from the places where external forces are applied, to eliminate the local irregularities produced by their application.

GEOLOGICAL SOCIETY.

[Continued from p. 401.]

March 7, 1866.—Warrington W. Smyth, Esq., President, in the Chair.

The following communications were read:—

1. "Documents relating to the formation of a new island in the neighbourhood of the Kameni Islands." By St. Vincent Lloyd, Esq., H.M. Consul at Syra, A. Delenda, Esq., Consular Agent at Santorino, and M. Décigala.

In these documents it was stated that on or about February 1st the sea in the neighbourhood of the Kameni Islands, in the centre of the crater forming the harbour of Santorino, began to show signs of volcanic action, and that the result has been the formation of a new island, which has since become nearly joined to the south of the island Nea Kameni. Details of the volcanic phenomena observed up to February 7th were given in the letters from Messrs. Lloyd and Delenda; and in the impression of 'La Grèce' newspaper of February 15th, M. Décigala gave an account of the further progress of the upheaval and increase of the new island, which he had named "George the First."

2. "On the Carboniferous Slate (Devonian Rocks) of North Devon and South Ireland." By J. Beete Jukes, Esq., M.A., F.R.S., F.G.S.

Mr. Jukes gave a sketch of the geological structure of the south-west of Ireland, tracing the Old Red Sandstone and Carboniferous Limestone from Wexford through Waterford into Cork, and showing that some thin beds of black shale which intervened between those groups on the east expanded westwards until they acquired

a thickness of two or three thousand feet. The group then received Sir R. I. Griffith's appellation of Carboniferous Slate. The Old Red Sandstone similarly expanded to the west, from a thickness of a few hundred feet in Wexford to one of several thousand feet in North Cork. Slaty cleavage, which shows itself in both these groups on the east, becomes more marked on the west, until they might both be spoken of as clay-slate formations.

West of Cork Harbour the Carboniferous Slate is covered in one or two places by patches of black slate, which Mr. Jukes believes to be the lower part of the Irish Coal-measures, resting conformably on the Carboniferous Slate, with no very definite boundary between the two, as in North Devon. He believes, accordingly, that as the Carboniferous slate expands towards the west, the limestone dies away from below upwards, and that the beds which are the base of the limestone at Ballea, a few miles west of Cork Harbour, are on the same horizon as the Upper Limestone beds of Waterford, even these uppermost beds disappearing a little further west. He therefore looks on the Carboniferous Slate, especially in its upper part, as being contemporaneous with the Carboniferous Limestone. He believes that there is a regular consecutive series from the Old Red Sandstone into the Coal-measures, through the Carboniferous Limestone in one area, and through the Carboniferous Slate in the other.

In North Devon the author considered the dark slates and sandstones which strike from Baggy Point and Croyde Bay by Marwood and Barnstaple to Dulverton, identical with parts of the Carboniferous slate of Ireland; and red and variegated sandstones and slates, rising out to the northward from underneath the grey series, identical in character with the Irish Old Red Sandstone. On a recent visit, however, to the north coast at Lynton, Ilfracombe, and Morte-hoe, he found that those beds also were identical in character with parts of the Carboniferous slate, and contained some of the same fossils as are found in Ireland and in the Barnstaple district, each district having fossils not found in the others.

These beds, in the absence of any reason to suspect the contrary, would be judged to pass under that Old Red Sandstone. Relying, however, on the conclusions formed in Ireland, Mr. Jukes believes that the central band of Old Red Sandstone must be cut off towards the north by a great longitudinal fault, with a downthrow to the north of some 4000 feet. Mr. Jukes pointed out that this hypothesis would do away with the necessity of assigning the enormous thickness to the North Devon rocks which the ordinary hypothesis requires.

After discussing in general terms the objections that may be urged against the hypothesis, especially the palæontological ones, he ventured to propose that geologists should no longer include the Old Red Sandstone among the Devonian beds, but confine the term Devonian to beds containing undoubted marine fossils, lying between the top of the Old Red Sandstone and the base of the Coal-measures. Taking their wide range over the world into account, he would even regard the Devonian rocks and fossils as the most general type of that portion of our series, and consider the Carbo-

niferous Limestone of the British Islands and Belgium the local and exceptional peculiarity.

March 21.—Warrington W. Smyth, Esq., President, in the Chair.

The following communications were read :—

1. "On the Fossil British Oxen.—Part I. *Bos Urus*, Cæsar." By W. Boyd Dawkins, Esq., M.A., F.G.S.

The problem of the origin of our domestic races of cattle was considered by the author to be capable of solution only after a careful examination of each of the three European fossil species of Ox, namely, *Bos Urus* of Cæsar, *B. longifrons* of Owen, and *B. Bison* of Pliny. In this paper he began the inquiry with *Bos Urus*, Cæsar, being the *Bos primigenius* of Bojanus; and he arrived at the conclusion that between this species and *Bos Taurus*, or the common Ox, there is no difference of specific value, though the difference in size and some other characters of minor value render the bones of the two varieties capable of recognition. After giving the synonymy of *Bos Urus* in some detail, and measurements of the different bones as represented by specimens from a number of localities, Mr. Boyd Dawkins described the range of the species in space and time, showing that it coexisted in Britain with the Mammoth, *Rhinoceros leptorhinus*, *R. megarhinus*, and *R. tichorhinus*, and was associated with *Elephas antiquus*, *Felis spelæa*, *Ursus spelæus*, *U. Arctos*, *Bos priscus*, *Megaceros Hibernicus*, *Cervus Elaphus*, *C. Tarandus*, *Equus fossilis*, &c., and held its ground during the Prehistoric period, after most of these animals had become extinct or retreated from this country. The precise date of its extinction in Britain was stated to be somewhat uncertain, although the author inclined to the belief that it existed in the wild state as late as the middle of the 12th century; while on the continent it seems probable that it lingered until the 16th century. The author then endeavoured to explain its gradual diminution in size by the progressive encroachment of cultivation on its old haunts; and in conclusion stated his belief that at least the larger cattle of Western Europe are the descendants of the *Bos Urus*, modified in many respects by restricted range, but still more by the domination of man.

2. "Further documents relating to the formation of a new island in the neighbourhood of the Kameni Islands." By Commander G. Tryon.

A detailed account was here given of the formation of the new island, named "Aphroessa" by the Greek Commissioners. It was stated to be 100 yards long by 50 wide, and to be daily increasing in size. Volcanic eruptions had taken place in two localities, one in the new island, and the other in what was called Mineral Creek, which is about two-fifths of a mile distant, and which had been completely filled up with lava. Considerable concussions were experienced at Patras and other parts of Greece, which were by some attributed to an earthquake, and by others to volcanic explosions; but with these exceptions no earthquake had attended the eruptions or the formation of the island.

3. "Note on the Junction of the Thanet Sand and the Chalk,

and of the Sandgate Beds and Kentish Rag." By T. McKenny Hughes, Esq., B.A., F.G.S.

At the bottom of the Thanet Sand there is always a bed of green-coated flints in a green and rust-brown clayey sand, which the author is of opinion was derived from the decomposition of the chalk by the percolation of carbonated water after the deposition of the Thanet Sand, as none of the flints are water-worn, and only chalk-fossils have been found in the bed.

At the base of the Sandgate Beds, and resting on rubbly Kentish rag, there is generally a bed of green sand; it may be seen in the quarries near Maidstone, where it occupies furrows of the nature of pipes. Mr. Hughes endeavoured to show that this bed has been derived from the decomposition of the Rag after the deposition of the brick-earth, and that the rubbly limestone below it is the same, in process of decomposition. He remarked in conclusion that conformabilities or unconformabilities of beds must not be inferred from an examination of the line of junction only, as that may have been very much modified after the deposition of the newer formation.

4. "On the Lower London Tertiaries of Kent." By W. Whitaker, Esq., B.A., F.G.S.

This paper gave the general results of the Geological Survey work in the Tertiary district of Kent, chiefly by the author, who expressed his agreement with Mr. Prestwich's paper, except in a few matters of mere detail.

Five different members of the Thanet Beds were distinguished, the only constant one being the "base-bed," the possible formation of which, after the deposition of the sands, &c. above, worked out in detail by Mr. Hughes, had occurred also to the author. It was shown that the fine Thanet Sand of West Kent was replaced eastward by beds of fossiliferous sandy marl and sand, which come on in succession above it.

Of the overlying Woolwich Beds, only the lower part is present in the eastern part of the county, the middle (the estuarine shell-beds) and upper parts being limited to the western and central districts.

The sands of East Kent which Mr. Prestwich had somewhat doubtfully classed with the basement bed of the London Clay and the pebble-beds of West Kent, part of which had been classed with the Woolwich series and part with the basement bed, the author had been led to look upon as a distinct division, to which he gave the name "Oldhaven Beds," and which are separable alike from the London Clay above and from the Woolwich Beds below.

The basement bed of the London Clay, in the limited sense in which it is understood by the author, changes its structure according to that of the underlying beds.

The author corrected some mistakes that had been made in a paper printed in the Society's Journal, in which an undoubted Eocene-bed near Chislet was classed with the Crag, on the strength of its fossils, many of which he believed to have been wrongly named.

It was then pointed out that the Woolwich and Reading series

was known to be transgressive over the underlying Thanet Beds, and it was shown that the Oldhaven Beds were transgressive over both, so that outliers of the last might rest at once on the Chalk; and from this the author thought that, in the absence of good palæontological evidence, the occurrence of the isolated patches of sand on the North Downs, which Mr. Prestwich had classed with the Crag, might be explained by the geological structure of the older Tertiaries, although he did not attempt to say that they belonged to that series: should a more decided opinion be given on their fossils, he was quite willing to take Mr. Prestwich's view.

April 11.—Warrington W. Smyth, Esq., President, in the Chair.

The following communications were read:—

1. "On the Brown Cannel or Petroleum Coal-seams at Colley Creek, New South Wales." By William Keene, Esq., F.G.S.

In this paper the author described the geological position of the Brown Cannel or Petroleum-coal of Colley Creek, Liverpool Plains. From an examination of the rocks, he stated that he had been able to determine that this Cannel is below the Coal-seams worked in the Newcastle coal-field. It appeared to form the very base of the Coal-measures, and to be in such close contact with the Porphyries, that these latter seemed mixed up with the lower portion of the Cannel Coal. There are two parallel seams of workable thickness, which are tilted at a high angle, and run north and south. In appearance the specimens are identical with the Brown Cannels from Hartley, and are but little different from the Boghead coal of Scotland.

At Scone, near the Kingdon Ponds, a section was noticed in which the marine fossiliferous bed is proved to overlies the coal-seams, affording, as the author remarks, conclusive testimony as to the high antiquity of the Coal-beds.

2. "On the Occurrence and Geological Position of Oil-bearing Deposits in New South Wales." By the Rev. W. B. Clarke, M.A. F.G.S.

The author first described the oil-producing schists and cannels of New South Wales as they exist at Colley Creek, at the head of the Cordeaux river (Illawarra shales), at various places in the Wollondilly and Nattai valleys, at Reedy Creek (Hartley Cannel), Stony Creek, and elsewhere; as well as a substance resembling "Bog-butter," occurring at Bournda, and probably of very recent date. Respecting the Colley Creek Cannel described in the previous paper, Mr. Clarke observed that he saw no porphyry near it, but that a seam or mass of the Cannel, which here contains numerous scarcely rounded grains of quartz, was passed through in the midst of a series of layers of black, partly unctuous clay, which also contained many similar quartz-grains; these grains gave to the clay a porphyritic aspect, so that by sight alone one might be led to consider them a decomposed porphyry. The chief conclusions at which the author arrived were, (1) that, with the exception of the Stony Creek Cannel, all the oil-producing deposits occur in the Upper Coal-measures, and that the Cannel of Stony Creek, on the

river Hunter, occurs in the Lower Coal-measures, which are above the Lower Marine beds with Trilobites, below which again are numerous fossiliferous beds before the porphyry is reached; and (2) that the Cannel belongs to beds in which *Glossopteris* occurs, and therefore may be a slight additional evidence of their antiquity, as it is an analogue of the "Bog Head" Cannel of Scotland.

3. "Remarks on the Copper mines of the State of Michigan."
By H. Bauerman, Esq., F.G.S.

The author described briefly the different conditions under which native copper is found in the trappean belt of the Upper Peninsula of Michigan, on Lake Superior. The district in question is a narrow strip of ground about 140 miles long, and from 2 to 6 miles in breadth, made up of alternations of compact and vesicular traps, with subordinate beds of columnar and crystalline greenstones, conformably interbedded with sandstones and conglomerates. Three different classes of deposits are known,—namely, transverse or fissure lodes in the northern district; cupriferous amygdaloids and conglomerates following the strike, in the central or Portage district; and irregular concretionary lodes, also parallel to the bedding, in the southern or Antonagon district. In the fissure-veins copper occurs either spotted through the vein-stuff, or concentrated in comparatively smooth plates, or lenticular masses, of all sizes up to 500 tons. In the Antonagon lodes the masses are also large, but of much more irregular forms. In the Portage district, on the other hand, only small masses are found, the great production of the mines of this region being derived from the finely divided spots and grains interspersed through the amygdaloids and conglomerates.

After giving details of the phenomena observed in the lodes of particular mines, and a list of the principal alternations of minerals, chiefly zeolites with quartz, native copper, and calcite, the latter mineral being both newer and older than the copper, the author proceeded to notice the various hypotheses that may be framed for elucidating the occurrence of native copper in the Lake Superior traps. Two principal sources were indicated—the first on the supposition that protoxide of copper may have originally formed part of the felspathic component of the trap, or that the same rock may have contained sulphuretted compounds of copper mechanically intermixed; while according to the second view the overlying sandstones may have contained small quantities of copper-bearing minerals, in a similar manner to the Kupferschiefer and other Permian and Triassic rocks in Europe. Supposing the trappean rocks to have been percolated by solutions carrying the products of the alteration of such minerals, it was suggested that the reduction to the metallic state was mainly produced by the action of substances containing protoxides of iron, which by higher oxidation have given rise to the dark-red colour and the earthy ochreous substances found in the vein-matter. The causes producing the metalliferous deposits in the trap were stated to have evidently acted throughout the whole system; and the absence of copper from the compact beds is probably rather due to the absence of cavities fit for the reception of such masses, than to any difference in chemical composition.

LXIX. *Intelligence and Miscellaneous Articles.*

ON THE INFLUENCE OF THE ELECTRO-NEGATIVE ELEMENTS UPON THE SPECTRA OF THE METALS.

DIACON has published an interesting paper upon the influence of chlorine, bromine, iodine, and fluorine upon the spectra of various elements, and has confirmed and extended Mitscherlich's results. The memoir is preceded by a good historical introduction, and is illustrated by four excellent figures of spectra. The author sums up his results as follows:—

The chlorides of certain metals which are very rapidly decomposed in the gas-flame are at least partially volatilized in a chloridizing flame, and are therefore capable of producing spectra. These spectra in general differ from those which are observed with the oxides of the same metals in an oxidizing flame.

The chloride, bromide, iodide, and fluoride of the same metal placed in an oxidizing flame may give rise to bright lines, the position of which is different according to the salt examined. These lines, the duration of which is very variable, are always accompanied by the spectrum which we obtain with the oxide.

Since the lines which appear with the chlorides belong to spectra which characterize these salts when volatilized in a chloridizing flame, it is proper to regard the new rays which are presented by the bromides, iodides, and fluorides as forming part of the spectra which these compounds would give in flames which do not act upon them. Hence it follows, that if it were possible to make with these salts experiments similar to those made with the chlorides, we should have for the same element, barium for instance, five different spectra.

Diacon considers the influence of the electro-negative element upon the spectrum to be placed beyond a doubt by his experiments, as well as by Mitscherlich's. The spectra given by Bunsen and Kirchhoff for the alkaline earthy metals are mixtures of the spectra which are obtained when the oxide is ignited in an oxidizing flame, and those obtained by igniting the chlorides in a chloridizing flame. The spectra attributed by Mitscherlich to the chlorides are also in part mixtures of the same spectra. In the first case, however, the spectra of the oxides predominate, and in the second those of the chlorides. —*Ann. de Chim. et de Phys.* S. 4. vol. vi. p. 5. (From Silliman's *American Journal* for March 1866.)

ON THE DETERMINATION OF THE REFRACTIVE EQUIVALENT OF THE ELEMENTS. BY A. SCHRAUF.

Recent investigations have shown that the expression given by Newton and by Laplace for the refractive power,

$$m = \frac{n^2 - 1}{d},$$

holds good, and adequately represents the dependence of the propagation of light (expressed by the refractive exponent n) on the density d , if the dispersion is taken into account. Some time ago,

therefore, under the supposition that

$$n = A + \frac{B}{\lambda^2},$$

I gave the formulæ

$$M = \frac{A^2 - 1}{d}, \quad N = \frac{B}{d^2},$$

in which the first denotes the refractive, and the latter the dispersive power.

A knowledge of this function renders it possible to take into account the dependence of the propagation of light on chemical decomposition. It was found that, calling the product of the atomic weight P with m (that is, $Pm = M$) the *refractive equivalent*, M of a compound is the sum of the simple or multiple M of the constituents in the form

$$M(a + b + c \dots) = M(a) + M(b) + M(c) \dots,$$

in which the refractive equivalents M for the different states of aggregation of a substance are in simple multiple relations. The application of this law, especially to binary compounds, rendered it possible to deduce from the observations at hand (which the author by his own observations partly increased and partly supplemented) the refractive equivalent of a great number of elements, and to find a number which quantitatively establishes the optical character of a compound, just as the atomic weights do the chemical character.

Assuming the atomic weights ($H=1$, $O=16$), the following values of the refractive equivalents of thirty-three elements were found for their (g) gaseous or vaporous, (s) solid or liquid, or (m) metallic condition, the refractive equivalent of hydrogen being taken at unity.

Aluminium.....	<i>s</i>	5.85	Mercury.....	<i>s</i>	18.99
Antimony.....	<i>m</i>	76.35	”.....	<i>m</i>	99.37
Arsenic.....	<i>g</i>	4.09	Nitrogen.....	<i>s</i>	2.10
”.....	<i>s</i>	12.39	Magnesium....	<i>s</i>	7.38
Barium.....	<i>s</i>	10.98	Oxygen.....	<i>g</i>	1.98
Bismuth.....	<i>m</i>	81.62	Phosphorus....	<i>g</i>	4.85
Lead.....	<i>m</i>	89.50	”.....	<i>s</i>	18.88
Boron.....	<i>s</i>	6.00	Potassium.....	<i>s</i>	4.77
Bromine.....	<i>s</i>	10.86	Sodium.....	<i>s</i>	3.71
Carbon.....	<i>s</i>	5.06	Sulphur.....	<i>g</i>	3.96
Calcium.....	<i>s</i>	7.74	”.....	<i>s</i>	16.13
Cadmium.....	<i>s</i>	11.72	Selenium.....	<i>m</i>	30.11
Chlorine.....	<i>g</i>	5.56	Silver.....	<i>m</i>	34.09
Copper.....	<i>m</i>	18.01	Silicium.....	<i>s</i>	8.81
Iron.....	<i>m</i>	33.89	”.....	<i>m</i>	32.77
Fluorine.....	<i>s</i>	1.00 (?)	Strontium.....	<i>s</i>	8.50
Hydrogen.....	<i>g</i>	1.00	Titanium.....	<i>s</i>	31.98
Iodine.....	<i>s</i>	19.03	Zinc.....	<i>s</i>	7.87
Lithium.....	<i>s</i>	3.25	”.....	<i>m</i>	21.75
Mercury.....	<i>g</i>	7.95	Tin.....	<i>s</i>	19.88

The numbers thus obtained render it possible to establish several comparisons as to the similarity of the optical and chemical character of the elements.—Poggendorff's *Annalen*, January 1866.

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SUPPLEMENT TO VOL. XXXI. FOURTH SERIES.

LXX. *On the Origin of Muscular Power.* By Dr. A. FICK, Professor of Physiology, Zurich; and Dr. J. WISLICENUS, Professor of Chemistry, Zurich*.

IT is now a universally acknowledged fact that muscular action is brought about by chemical changes alone; and the proposition, that it is by processes of oxidation that the muscles are rendered capable of performing work, would be just as little likely to meet with contradiction. But all are not agreed as to what the substance is, which, by oxidation, furnishes the store of actual energy which is capable of being in part transmuted into mechanical work. Most physiologists and chemists appear to think that the oxidation of albuminoid substances alone can generate muscular power. Even quite recently Playfair† has published a special treatise to prove this hypothesis. The beautiful investigations of J. Ranke‡ seem also to point indubitably in the same direction. In many manuals of physiology the proposition in question is laid down as a self-evident principle. The chief reason why it numbers so many adherents may lie in the following reflection made, more or less consciously, by most of them:—The action of the muscle is connected with the destruction of its substance, by far the greater part of which is of an albuminoid nature; therefore the destruction by oxidation of albuminoid bodies is the essential condition of the muscle's mechanical action. The fallacy of this line of argument will be immediately apparent if, for instance, we apply it to a locomotive. "This machine consists essentially of iron, steel, brass, &c.; it contains but little coal; therefore its action must depend on the burning of iron and steel, not on the burning of coal." In like manner it is by no means self-evident that it is

* Communicated by the Authors, and translated by Professor Wanklyn.

† "On the Food of Man in relation to his useful Work," read to the Royal Society of Edinburgh, April 3, 1865.

‡ Tetanus, a Physiological Study. Leipzig, 1865.

specially the oxidation of albuminous compounds which produces muscular force. It is quite possible that the non-nitrogenous substances in muscle play the part of combustibles, although but a small quantity of those substances is to be found in muscle at any particular moment. It is even conceivable that these compounds pass, as it were, through the muscle in a rapid stream, each particle which enters it being immediately oxidized and carried away again. If we examine this supposition more closely, we find that, even from the most general points of view, there is much to be said for the hypothesis that non-nitrogenous compounds form the fuel or oxidizable materials in the muscles.

Liebig long ago pointed out that the non-nitrogenous organic compounds of the food, particularly the hydrates of carbon and the fats, are the sources of heat. He, indeed, could hardly have thought of fuel generating mechanical power, chiefly because our present problem was far beyond the chemists and physiologists of that time. But from the point to which science has at present attained, if once a certain group of materials of food are found to be heat-producers, it is easy to conceive the derivation from the oxidation of these substances, not merely of heat, but also of the mechanical work of the organism, since, as is now well known, heat and mechanical work are only two manifestations of the same force. In fact it would be very strange if, in the animal economy, one particular group of food-constituents was used merely for the production of heat, in order that the temperature of the body might be maintained above that of the surrounding medium. No doubt this animal warmth is a necessary condition of existence for mammals and birds; but the mechanical theory of heat teaches that the almost inevitable secondary result of the production of muscular work must be heat, and that therefore no special processes are necessary to raise the temperature of the organism, the latter, in fact, going hand in hand with the production of mechanical power. If the non-nitrogenous compounds were exclusively heat-producers in the narrower sense, and the albuminoid bodies, on the contrary, only force-producing material, nature would have proceeded as uneconomically as a manufacturer who should put up a stove near a steam-engine, although a sufficient amount of heat was given out by the steam-engine itself. Although at present, in the light of Darwin's theory, the employment of teleological arguments in a certain order of cases has once more become admissible, yet we are far from being of opinion that a chemico-physiological question like the above is capable of being decided by such arguments. Nevertheless considerations of this nature may serve to shake other hypotheses which rest only on a teleological basis.

The doctrine that muscular force is produced only by the oxidation of albuminoid compounds, is, however, much more seriously shaken by the important investigations of Edward Smith, who has shown, in the most convincing manner, that the production of carbonic acid in the human body may be increased ten-fold by muscular exertion, whilst the excretion of urea proceeds with tolerable uniformity. The latter fact has also been frequently observed by other inquirers, viz. by Bischoff and Voit (in part before the investigations of E. Smith). The numbers given by Smith do not, however, furnish quite a direct disproof of the doctrine in question. If any adherent of that doctrine felt inclined to retain it on any terms, he might reply to Smith, "Probably muscular action necessarily excites the process of oxidation of non-nitrogenous substances without these compounds having anything to do with the production of that action." On the other hand, the objection might be made to Smith, that perhaps, when there is violent exertion of the muscles, though the metamorphosis of compounds containing nitrogen is increased, yet the secretion of urea is not larger, because the results of the metamorphosis of these compounds leave the body in other forms.

There is one way in which the question whether muscular force can be generated only by the oxidation of albuminoid compounds might be decisively negatived, and that possibly by a single experiment. It is suggested by the following simple line of thought. Granting that a person might accomplish a certain measurable amount of external labour, say m metre-kilogrammes, and that in so accomplishing it he oxidized p grammes of albumen in his muscles; granting also that we know the amount of heat which is liberated when a gramme of albumen is changed by oxidation into the products of decomposition in which the constituents of albumen leave the human body; then if the thermic equivalent of the manual labour m be greater than the amount of heat which could possibly be produced by the oxidation of p grammes of albumen, the question may be negatived with the most complete certainty. But if, on the contrary, the thermic equivalent of m metre-kilogrammes is less than that of the heat arising from the oxidation of p grammes of albumen, the question has by no means received an affirmative answer. It is only in the former case that the experiment has a decisive result.

Such an experiment has been made by us conjointly. It is true that the quantities which require to be determined, with the exception of the mechanical work, cannot have any exact numerical value assigned to them; but we can confine their values within certain limits, so that a satisfactory conclusion

may nevertheless be obtained. As measurable external labour we chose the ascent of a mountain-peak, the height of which was known. We preferred the mountain to a *treadmill*, not merely because the ascent is a more entertaining employment, but chiefly for the reason that we had no suitable treadmill at our disposition. Of the numerous peaks of the Swiss Alps, the one most suitable for our purpose appeared to be the Faulhorn, near the lake of Brienz, in the Bernese Oberland. It was necessary that the mountain which was to serve for our experiment should be as high as possible, and nevertheless should permit of our passing a night on its summit under tolerably normal circumstances; for had we been obliged immediately to descend again, the measurable amount of work would have been at once followed by an undeterminable but violent exertion of the muscles, in which much metamorphosis would occur, the thermic equivalent of which would be, however, entirely liberated as heat. The Faulhorn satisfies all these requirements; for although its height is very considerable, rising to about 2000 metres above the lake of Brienz, yet there is an hotel on its summit. Besides, it can be ascended by a very steep path, which was, of course, favourable for our experiment, because the amount of muscular action which is lost and not calculable (being reconverted into heat) is thus reduced to a minimum. We chose the steepest of the practicable paths; it starts from a little village on the lake of Brienz called Iseltwald, and at a moderate pace the summit may be reached in less than six hours.

In order to diminish as far as possible the unnecessary consumption (*Luxusconsumtion*) of albumen during our experiment, we took no albuminoid food from midday on August 29 until 7 o'clock in the evening of August 30. During these thirty-one hours we had nothing in the way of solid food except starch, fat, and sugar. The two former, following F. Ranke's directions, were taken in the form of small cakes. Starch was rubbed up with water, and the thin paste thereby produced was fried with plenty of fat. The sugar was taken dissolved in tea. There was also the sugar contained in the beer and wine, which were taken in the quantity usual on foot tours.

The experiment proper began on the evening of the 29th of August, at a quarter past 6 P.M., by a complete evacuation of the bladder. The urine from this time till 10 minutes past 5 on the morning of the 30th was collected and measured; we will call it *night urine*; we took a specimen of it with us for examination. Then the urine secreted in the time between 10 minutes past 5 in the morning and 20 minutes past 1 in the afternoon of the 30th of August was also measured. During this time the ascent of the mountain was performed, it may therefore be designated

the *work urine*. Again, the amount of urine secreted between 20 minutes past 1 and 7 in the evening was determined; this we will call simply the *after-work urine*. During this time we remained, for the most part, in the house without any muscular exertion worth mentioning. After 7 o'clock a plentiful meal, consisting chiefly of meat, was taken, and lastly the urine excreted during the night spent in the hotel of the Faulhorn (that is to say, from 7 o'clock in the evening of the 30th till half-past 5 in the morning of the 31st of August) was measured and again designated *night urine*. Arrived at the hotel on the summit of the Faulhorn, determinations of the quantity of urea in the specimen of the *night urine* from the 29th to the 30th, and of the *work urine* excreted between half-past 5 in the morning and 20 minutes past 1, shortly after the end of the ascent, were proceeded with. The method followed was precisely that given by Neubauer for absolute determinations of urea.

Phosphoric and sulphuric acids were first precipitated from 40 cubic centims. of urine by 20 cubic centims. of solution of baryta, which quantity was proved to be sufficient; and 15 cubic centims. of the filtrate (=10 cubic centims. of urine) were employed for the determination of chlorine by Liebig's method, with a solution of nitrate of mercury of which 1 cubic centim. corresponded to 0.01 grm. of common salt. From 30 cubic centims. of the filtrate the chlorine was then precipitated with the exact amount of silver solution necessary, as indicated by the amount of mercury solution previously employed. A portion of the filtrate (= to 5 cubic centims. of urine) was then employed in making an approximative determination of the amount of urea, and with another portion (= the 10 cubic centims. of urine) an exact determination of urea was made with the necessary mercury solution (1 cubic centim. = 0.01 grm. of urea). These operations were performed with every possible precaution.

On the morning of the 30th of August we made similar determinations of the *after-work urine* of the preceding day, collected between 20 minutes past 1 P.M. and 7 o'clock in the evening. A specimen of each kind of urine was also sealed up in a perfectly filled and well-corked and sealed bottle, in order that on our return to Zurich the yet more important determination of the absolute quantity of nitrogen might be undertaken; the same thing was done with the second *night urine*, collected and measured between 7 o'clock in the evening of the 30th of August, and 6 o'clock in the morning of the 31st; but the quantity of urea in this, owing to want of time, was not determined. The chlorine and urea determinations gave the following results:—

I. Night urine from the 29th to the 30th of August. In both cases coloured light yellow, perfectly clear, decided acid reaction.

(a) Fick : total quantity of urine 790 cubic centims. In 10 cubic centims. we found 0.0619 grm. of common salt, and, regard being had to the necessary corrections*, 0.1580 grm. of urea. The total quantity of the latter amounted, consequently, to 12.4820 grms.

(b) Wislicenus : quantity of urine 916 cubic centims. In 10 cubic centims. 0.03 grm. of salt and 0.1284 grm. of urea; in the whole quantity, therefore, 11.7614 grms. of the latter.

II. Work urine, from half-past 5 in the morning of the 30th of August till 20 minutes past 1.

(a) Fick : quantity of urine 396 cubic centims.; pale yellow, clear, acid. In 10 cubic centims. 0.0395 grm. of salt and 0.1776 grm. of urea. Total quantity of urea 7.0330 grms.

(b) Wislicenus : quantity of urine 261 cubic centims.; pale yellow, turbid after cooling, acid. In 10 cubic centims. 0.0460 grm. of salt and 0.2566 grm. of urea. Total quantity of urea 6.6973 grms.

III. The after-work urine, from 20 minutes past 1 on the 30th of August till 7 in the evening. In both cases darkish yellow, a sediment being deposited on cooling, acid.

(a) Fick : quantity of urine 198 cubic centims. In 10 cubic centims. 0.007 grm. of salt and 0.2612 grm. of urea. Total quantity of urea 5.1718 grms.

(b) Wislicenus : quantity of urine 200 cubic centims. In 10 cubic centims. 0.018 grm. of salt and 0.2551 grm. of urea. Total quantity of urea 5.1020 grms.

It is evident that the amount of urea found cannot be the correct measure for the quantity of oxidized albuminoid substances, particularly as a portion of nitrogen, which must not be omitted, must always have been present in the sediment, which consisted almost exclusively of acid urate of soda. It therefore became necessary to ascertain the total quantity of nitrogen present. The urea determinations were undertaken principally in order that, if any accident should happen to any of the specimens of urine on their way to Zurich, there might be at least some data saved for an approximate calculation of the nitrogen.

The absolute nitrogen determinations were made between the 4th and 6th of September, in the laboratory of the Zurich University, with the urine which had remained perfectly fresh. 5 cubic centims. of it were distilled with excess of soda-lime, from a suitable apparatus, into pure hydrochloric acid, the residue heated till it became colourless, and air drawn through the apparatus and through the acid. The determination of the am-

* Neubauer and Vogel, *Analyse des Harnes*, 4 Aufl. p. 143-146.

monia absorbed by the acid was made as usual with perchloride of platinum, the double salt so obtained ignited and weighed, the quantity of nitrogen being calculated from the weight of the metallic platinum found.

I. Night urine from the 29th to the 30th of August.

(a) Fick: 5 cubic centims. of urine gave 0.3095 grm. platinum = 0.043768 grm. nitrogen. Total quantity of nitrogen 6.915344 grms.

(b) Wislicenus: 5 cubic centims. of urine gave platinum 0.2580 grm. = 0.036485 grm. nitrogen. Total quantity of nitrogen 6.684052 grms.

II. Work urine.

(a) Fick: 5 cubic centims. of urine gave platinum 0.2958 grm. = 0.418303 grm. nitrogen. Total quantity of nitrogen 3.312960 grms.

(b) Wislicenus: 5 cubic centims. of urine gave platinum 0.4245 grm. = 0.060030 grm. nitrogen. Total quantity of nitrogen 3.133566 grms.

III. The after work urine.

(a) Fick: 5 cubic centims. of urine gave platinum 0.4338 grm. = 0.06134545 grm. nitrogen. Total quantity of nitrogen 2.4293 grms.

(b) Wislicenus: 5 cubic centims. of urine gave platinum 0.4272 grm. = 0.0604121 grm. nitrogen. Total quantity of nitrogen 2.416484 grms.

IV. Night urine from the 30th to the 31st of August.

(a) Fick: 5 cubic centims. of urine gave platinum 0.6601 grm. = 0.0933475 grm. nitrogen. Total quantity of nitrogen in the 258 cubic centims. 4.816731 grms.

(b) Wislicenus: 5 cubic centims. of urine gave platinum 0.7001 grm. = 0.099004 grm. nitrogen. Total quantity of nitrogen in the 270 cubic centims. of urine 5.346216 grms.

From these figures result the following comparative Tables, for which the amount of nitrogen in the total quantity of the urea has been calculated:—

Fick.

	Urea.	Nitrogen in the urea.	Total nitrogen.
Night urine, 29th to 30th of August . . . }	12.4820	5.8249	6.9153
Work urine	7.0330	3.2681	3.3130
After-work urine . . .	5.1718	2.4151	2.4293
Night urine, 30th to 31st of August . . . }	4.8167	4.8167	4.8167
			5.7423

Wislicenus.

	Urea.	Nitrogen in the urea.	Total nitrogen.
Night urine, 29th to 30th of August .	11·7614	5·4887	6·6841
Work urine . . .	6·6973	3·1254	3·1336
After-work urine .	5·1020	2·3809	2·4165
Night urine, 30th to 31st of August	5·3462

It is evident that the calculation of the amount of the protein substances in question must be based upon the numbers for the total amount of nitrogen, and therefore that, in order to render the result as convincing as possible, the most unfavourable case (that is, the one which leads to the greatest quantity of protein bodies) must be selected. All real tissue-forming protein bodies, with the exception of permanent cartilage yielding chondrin, contain more than 15 per cent. of nitrogen: we are therefore entitled to make this proportion the basis of our calculation, and we thereby obtain the following numbers for the protein bodies consumed:—

	Fick. grms.		Wislicenus. grms.
For night urine, 29th to 30th of August }	46·1020		44·5607
For work urine . .	22·0867	grms.	20·8907
For after-work urine	16·1953	38·2820	16·1100
For night urine, 30th to 31st of August }	32·1113		35·6413

Let us, in the first place, use the figures we have obtained, in order to gain an idea of the course of the excretion of nitrogen through the urine during the period of the experiment. For this purpose we divide the amounts of nitrogen given in the first two Tables by the number of hours during which they were secreted, and we obtain,

Average quantity of Nitrogen excreted per hour.

	By Fick. gm.	By Wislicenus. gm.
During the night, 29th to 30th .	0·63	0·61
During the time of work . . .	0·41	0·39
During rest after work . . .	0·40	0·40
During the night, 30th to 31st .	0·45	0·51

A glance at these Tables furnishes a new testimony to the fact, which has often been before experimentally proved, that muscular exertion does not notably increase the excretion of nitrogen through the urine. It declined in our experiment tolerably regularly from the 29th of August till the evening of the 30th*, evidently in consequence of abstinence from food containing nitrogen. In the night of the 30th to the 31st, in spite of the plentiful meal of albuminous food on the evening of the 30th, the secretion of nitrogen was less than on the preceding night. The reason of this perhaps was that during abstinence the secretion of nitrogen (never entirely discontinued) was carried on at the expense of tissues, and now these tissues had first of all to be repaired. We will not pursue such considerations further, but will apply our figures to other conclusions.

We must, it is true, in the first place take our stand upon an hypothesis, which, however, has been well established by many recent investigations. We assume, namely, that the nitrogen of the oxidized albumen leaves the body entirely through the urine. In fact it has lately been proved, on the one hand by Ranke, on the other by Thiry, that neither by the perspiration nor by the breath is any perceptible quantity of nitrogen disposed of. Fortunately we are further in a position to state that during the ascent of the mountain we neither of us perspired to a perceptible degree. During the whole ascent we were enveloped in a cold mist, which prevented us from becoming overheated. Even if any noticeable amount of nitrogen were excreted with the fæces, we should yet be justified in the preceding experiment in neglecting it; for the nitrogenous products of the transmutation of albumen which might possibly be contained in the fæces are doubtless not highly oxidized compounds, and no heat worth mentioning is liberated in their production.

We have now to consider, on the preceding assumptions, what is the largest amount of albumen that can have been oxidized in our bodies during the ascent of the mountain. We think we should be justified in not estimating the albumen oxidized during the hours of work higher than was calculated from the quantity of nitrogen excreted in the work urine (viz. 22.09 for Fick, 20.89 for Wislicenus). In fact the rate of the nitrogen excretion seems to be so entirely regulated by the supply of food, and so completely independent of muscular action, that we can reasonably suppose it to depend only upon the decomposition of protein substances. If any one were to maintain that at the

* The slight apparent deviation of the numbers under Wislicenus ought hardly to come into consideration, as they may easily have been affected by the retention of some urine in the bladder, or by other similar disturbances.

end of the time of work any considerable quantity of the nitrogenous products of decomposition remained in the body, we might reply that at least an equal quantity of such products must have been in the body at the commencement of the time of work.

We will not, however, insist upon this point; nay, we will even concede to the opponents of our hypothesis that there was an exceptionally large quantity of the peculiar nitrogenous products of muscular action retained in the body. We will also not avail ourselves of the consideration that this curious phenomenon, if it really existed, would only indicate that the products of decomposition resulting from muscular action were not so highly oxidized as ordinarily, and that therefore comparatively little heat was liberated during their formation. We will, as we have said, put aside all these considerations; but we may, without danger of meeting with any opposition, assume that in the six hours following the time of work an amount of the nitrogenous products of decomposition was discharged, at least, as great as the difference between the quantity in the body at the end of the time of work and that which was in it at the commencement of that period; in fact, actual data for this assumption are not altogether wanting. Among the products of the decomposition of protein substances, the one which alone suggests itself as likely to be retained in noticeable quantities in muscle is creatin. Now observation, it must be admitted, goes to prove that a muscle which has been hard worked contains more creatin than one which has been at rest. Thus the quantity of creatin contained in the heart of an ox was found to be 0.0014 (Gregory), and that in other ox-flesh only 0.0006 (Städeler). Let us now suppose that in our case the extensor muscles of the thigh, which really do the essential work in ascending, contained, previously to that exertion, the same quantity of creatin, 0.0006, as ordinary ox-flesh, but after it as much as is found in an ox's heart; then the difference between these two should be added to the amount excreted through the urine during the time of work. Now the weight of the muscles which extend the leg in walking is estimated in a full-grown and powerful man at 2913 grms. (see Weber, *Mechanik der Gehwerkzeuge*, p. 218); and the muscles of the two legs would therefore weigh 5.8 kilograms. According to these data the surplus of creatin exceptionally retained after the work will be 5.8 kilograms. $(0.0014 - 0.0006) = 4.64$ grms.; this indicates 8.4 grms. of albumen.

From the products of decomposition discharged during the six hours following the time of work we have reckoned over 16 grms. of albumen; we may therefore certainly assume that, during those six hours, at least as much of the decomposition products

of protein substances appeared in the urine as the surplus which might possibly have been retained after work, over and above that normally contained in the tissues. This being granted, we have the data for further calculations, in the total amount of nitrogen contained in the work urine and after-work urine (see page 492). This is for Fick 38.28 grms., and for Wislicenus 37.00 grms. of decomposed albumen. To the first of these two figures we may apply a slight correction. For obvious reasons, we were compelled to measure the night urine at our hotel at Interlaken at 10 minutes past 5. But the work really began two hours later, after one hour passed on the steamboat, and another spent over breakfast at Iseltwald; therefore the urine secreted during these two hours ought not properly to have been reckoned with the work urine. In the case of one of us, however (Fick), this error may be in some measure repaired; just before the ascent he had discharged a quantity of urine into the vessel destined to receive the work urine. This quantity could not indeed be measured, because the graduated apparatus had been packed up at Interlaken, but, by the eye, it must have been at least as much as 20 cubic centims. If we assume that this quantity of urine contained the same proportion of nitrogen as the work urine, it would correspond to 1.11 grm. of albumen, which we ought without doubt to deduct from the number 38.28. We thus obtain for the greatest possible quantity of albumen oxidized in Fick's body during the ascent 37.17 grms.

The question now arises, what quantity of heat is generated when 37.17 and 37.00 grms. of albumen are respectively burnt to the products in which their constituent elements leave the human body through the lungs and kidneys? At present, unfortunately, there are not the experimental data required to give an accurate answer to this important question; for neither the heat of combustion of albumen nor of the nitrogenous *residue* of albumen is known. We are able, however, to assign a limit which the quantity of heat in question will not overstep. In fact it is quite certain that the amount of heat which 1 grm. of albumen will give when completely burnt must be less than the amount of heat which would be obtained if the combustible elements contained in a gramme of albumen were burnt separately; that is, in other words, the heat of combustion of albumen is less than that of a mixture of the elements in the same proportions, and not in combination with oxygen. Now the latter number may be easily calculated; it is only requisite to determine how much heat would be produced by the combustion of the amount of carbon and hydrogen contained in a gramme of albumen. The nitrogen may be neglected, since it is

known to be set free in the combustion of albumen. Very probably also nitrogen has no heat of combustion at all; that is to say, in the combination of an atom of nitrogen with oxygen there is not so much heat developed as is consumed in the separation of the molecules of oxygen into the atoms of which they are composed. A gramme of albumen contains 0.535 grm. of carbon, which, computed at the highest rate (8080) for the heat of combustion of carbon, gives 4.32 units of heat. For the combustion of the 0.07 grm. of hydrogen contained in 1 grm. of albumen, putting 34,462 for the heat of combustion of this element, there are 2.41 units of heat; altogether for 1 grm. of albumen 6.73 units of heat. The true heat of combustion of a gramme of albumen is therefore in any case considerably less than this number; and still smaller must the amount of heat be which is liberated by the imperfect oxidation of a gramme of albumen as it actually occurs in the human body. Conceding this point to our opponents, and making the almost absurd assumption that 6.73 units of heat are produced by the consumption of 1 grm. of albumen in the human body; then there would arise from the consumption of albumen, in the case of Fick, $37.17 \times 6.73 = 250$, and in that of Wislicenus $37.00 \times 6.73 = 249$ units of heat, available for the muscular work done in ascending the mountain. Expressed in units of work, this gives for Fick 106,250, and for Wislicenus 105,825 metre-kilogrammes.

Let us now inquire how much work was really done by our muscles. One item necessary for the reply is already at hand, viz. the height of the summit of the Faulhorn above the level of the lake of Brienz multiplied by the weight of the body; the former reckoned in metres, the latter in kilogrammes. The weight of the body with the equipments (hat, clothes, stick) amounted to 66 kilogrammes in Fick's case, and 76 in Wislicenus's. The height of the Faulhorn above the level of the lake of Brienz is, according to trigonometric measurements, exactly 1956 metres. Therefore Fick performed 129,096 and Wislicenus 148,656 metre-kilogrammes of muscular work.

The question we put at the outset is in this manner definitively answered. *The burning of protein substances cannot be the only source of muscular power; for we have here two cases in which men performed more measurable muscular work than the equivalent of the amount of heat, which, taken at a most absurdly high figure, could be calculated to result from the burning of the albumen.* But our proof becomes far stronger when we consider that a much greater amount of heat must have been given forth by the force-producing chemical processes than is equivalent to the measurable external work performed. In the

first place we may add to the data already obtained of 129,096 and 148,656 metre-kilogrammes another item which can be expressed in units of work; and though its value cannot be quite accurately calculated, yet a tolerable approximation can be made. It consists of the force consumed in respiration and the heart's action. The work performed by the heart has been estimated, in a healthy full-grown man, at about 0.64 metre-kilogramme* for each systole. During the ascent, Fick's pulse was about 120 per minute. That gives for the 5.5 hours of the ascent an amount of work which may be estimated at 25,344 metre-kilogrammes, entirely employed in the maintenance of the circulation. No attempt has yet been made to estimate the labour of respiration. One of us has shown, however, in the second edition of his 'Medical Physics' (p. 206), that Donder's well-known investigations concerning the conditions of pressure in the cavity of the thorax give sufficient data for such an estimate. He has there shown that the amount of work performed in an inspiration of 600 cubic centims. may be rated at about 0.63 metre-kilogramme. Fick breathed during the ascent at an average rate of about 25 respirations per minute, which gives, according to this estimation, an amount of respiratory work for the whole ascent of 5197 metre-kilogrammes. If we add this, and the number representing the work of the heart, to the external work performed by Fick, we obtain a total of 159,637 metre-kilogrammes, which is already half as much again as the amount of heat to be obtained by the burning of the albumen. In the case of Wislicenus, the relative amounts are still more striking. If we suppose that his respiratory and circulatory work bore the same proportion to Fick's as his bodily weight did to Fick's, *i. e.* 7:6, we obtain for Wislicenus's amount of work, as far as it is possible to calculate it, a total of 184,287 metre-kilogrammes, which exceeds that calculated from the oxidation of the proteine substances by more than three-fourths.

Besides these estimated (and certainly not over-estimated) items, there are several others which cannot be even approximately calculated, but the sum of which, if it could be obtained, would probably exceed even our present large total. We will try to give at least some sort of an account of them. It must first be remembered that in the steepest mountain path there are occasional level portions, or even descents. In traversing such places the muscles of the leg are exerted as they are in ascending, but the whole work performed is transformed back into heat. The same force-producing process, however, must be going on in the

* 0.43 is here assigned as the work of the left, and 0.21 as that of the right ventricle.

muscles as if work were being performed which did not undergo this transformation. In order to make this point yet clearer, we may take into consideration that the whole work of the ascent only existed temporarily as work. On the following day the result was reversed; our bodies approached the centre of the earth by as much as they had receded from it the day before, and, in consequence, on the second day an amount of heat was liberated equal to the amount of work previously performed. The two parts of the action, which in this case were performed on two separate days, take place in walking on level ground in the space of a footstep.

Let us observe, besides, that in an ascent it is not only those muscles of the leg specially devoted to climbing which are exerted, the arms, head, and trunk are continually in motion. For all these movements force-generating processes are necessary, the result of which cannot, however, figure in our total of work, but must appear entirely in the form of heat, since all the mechanical effects of these movements are immediately undone again. If we raise an arm, we immediately let it drop again, &c.

There was besides a large portion of our muscular system employed during the ascent, which was performing no external work (not even temporary work, or mechanical effects immediately reversed), but which cannot be employed without the same force-generating processes which render external work possible. As long as we hold the body in an upright position, individual groups of muscles (as, for instance, the muscles of the back, neck, &c.) must be maintained in a state of continual tetanus in order to prevent the body from collapsing. This point seems to have been much misunderstood; and we will therefore discuss it, shortly but fundamentally. The reason of this misunderstanding is, that in many treatises on the subject such employment of the muscles is denominated "statical work," although there is really no *work* when a tetanized muscle holds a burden in equilibrium. It is only at the commencement of this condition, when the burden is raised, that there is any work. We might therefore, in order to correct this misunderstanding, and shortly to designate the condition in question, propose the expression "statical activity." While the tetanized muscle is maintaining a burden in equilibrium, it is certainly in an active state. As long as it supports the burden, the force-generating processes must be active in it; but the whole of the actual energy thereby produced must necessarily be liberated in the form of heat; for no work is really performed. It may assist in comprehending this if we put before the reader a perfectly similar and more simple case. Let us imagine a cylinder standing perpendicularly and closed at the bottom; it is filled with some kind of gas, and the whole is in

equilibrium with the surrounding air. In the cylinder is a piston fitting air-tight, but moveable without friction. The piston has a certain weight, which we will imagine at first to have balanced the elasticity of the gas underneath it: now let us apply to the gas in the cylinder a certain amount of heat so that its temperature rises; then the balance between the elasticity of the gas and the weight of the piston will be destroyed. The latter will rise. Here is a certain amount of external work done, and a certain corresponding portion of the added heat disappears. If we now leave the apparatus alone, the rest of the added heat will gradually be abstracted by the surrounding air, and the piston will sink to its old place. It may be observed, in passing, that during this process the heat which had been transmuted into work again appears as heat. We might now propose to ourselves the task of keeping the piston up in the cylinder. To do this the increased temperature of the gas must be maintained. This can only be done by a continual addition of heat to it; for, according to the conditions of the experiment, it would always be losing heat. Evidently then no more than an exact compensation for the loss is needed to maintain the elevation *ad infinitum*. No more heat will be changed into work, because no more work is done. Suppose, for instance, we produced the warmth necessary to maintain the position of the piston by burning carbon, the whole of the heat produced by its burning would now be liberated, and diffused into the medium surrounding the cylinder. We may conceive of a tetanized muscle (like the heated gas in the cylinder) as holding up a weight which would immediately fall if the supply of actual energy were to cease. It is active, but it performs no work, and therefore all the force produced is liberated in the form of heat.

If we return from this digression to our subject, we shall find that we have another and a last item to add to the total of actual energies which must be provided by force-generating processes in muscles. It is hardly conceivable, on the principles of the mechanical theory of heat, that these processes, even in the case of real muscular work, produce only just the quantity of actual energy needed for the mechanical work in question. It is, on the contrary, tolerably certain that only a part of the actual energy developed by those force-generating processes can be transmuted into mechanical work. This exceedingly probable conclusion, deduced from the most general physical considerations, has been confirmed by experiment. From Heidenhain's* beautiful investigations concerning the connexion of the development of heat with muscular activity, it is possible to estimate, at least ap-

* *Mechanische Leistung und Wärmeentwicklung im Muskel*. Leipzig, 1864.

proximately, the value of the collective amounts of actual energy developed in single experiments; at least it is possible to fix a point below which it will not descend. This lowest possible value is almost always considerably higher than that of the equivalent of actual energy transmuted during the experiment into mechanical work. It may be casually remarked that in almost all Heidenhain's experiments, the mechanical work was changed back again into heat; for in every case the muscle allowed the weight it had raised to fall again, so that no mechanical work was really performed, and therefore the whole actual energy generated could appear only in the form of heat. It appears from Heidenhain's investigations, that the relation of that portion of the actual energy transmuted into work to the total energy produced in the contraction of muscle, varies very much according to the tension of the muscle while working. But we shall not be taking too high a figure if we assume that this proportion can never, under normal circumstances, be greater than as $\frac{1}{2}$ to 1. According to this we must at once double the numbers found above for the total amount of work permanently or temporarily performed, and should thus obtain a number which would give us an approximation to the sum total of actual energy (expressed in units of work) which must have been supplied by the force-generating processes in the muscle, in order that such work might be performed. The number which ought still to be added to these figures in order to represent the work which had been actually performed, and the statical activity of the muscles, would certainly be considerable; but it must be omitted from the calculation, because, as we said above, we have no data from which to compute it. We must therefore abide by the 319,274 metre-kilogrammes for Fick, and 368,574 for Wislicenus.

It may be thought that by making use of Helmholtz's well-known conclusion, we might much more easily have calculated a minimum for the total actual energy provided by the processes for the generation of muscular force during the ascent. By ingeniously combining the results of Smith's experiments on respiration with Dulong's determinations of animal heat, and with the well-established hypothesis concerning capability of work in man, that physiologist inferred that in the human body not more than one-fifth of the heat produced by the oxidation of metamorphosed substances is transmuted into muscular work. It would seem, according to this, that we might obtain the minimum value which we require simply by multiplying the estimated amount of muscular work by five. But this is not the case; for Helmholtz does not separate the processes which generate muscular power from the somewhat different ones which pro-

duce heat. - He looks upon the whole body as an apparatus working mechanically, and comes to the conclusion that this apparatus can only utilize one-fifth of the whole amount of heat generated in it by oxidation. By multiplying our external work by five, we obtain a minimum of the total amount of actual energy generated by all processes of oxidation during the ascent; but among these processes there might be some, such as the oxidation of the constituents of the circulating blood, which have nothing to do with the production of muscular power.

Let us therefore content ourselves with the figures which we previously obtained, and which will afford sufficient proof for our purpose. We had by their means arrived at the following result:—During our ascent, force-generating processes must have been carried on in our muscles sufficient to afford 751 units of heat in Fick's case, and 820 in Wislicenus's. But the actual amount of albumen oxidized could not, as we calculated, have afforded even a third of this quantity of heat. We therefore repeat, on far more satisfactory grounds, our former conclusion, viz. that the oxidation of albuminous substances cannot be the only source of muscular action. We can now go further, and assert that *the oxidation of albuminous bodies contributes at the utmost a very small quota to the muscular force.* From this assertion it is but a step, which we cannot avoid taking, to the doctrine which has already been frequently proclaimed more or less clearly*, and which has lately been advanced in a most decided manner by Traube, viz. *that the substances by the burning of which force is generated in the muscles, are not the albuminous constituents of those tissues, but non-nitrogenous substances, either fats or hydrates of carbon.*

We might express this doctrine by the following simile:—A bundle of muscle-fibres is a kind of machine consisting of albuminous material, just as a steam-engine is made of steel, iron, brass, &c. Now, as in the steam-engine coal is burnt in order to produce force, so in the muscular machine, fats or hydrates of carbon are burnt for the same purpose. And in the same manner as the constructive material of the steam-engine (iron, &c.) is worn away and oxidized, the constructive material of the muscle is worn away, and this wearing away is the source of the nitrogenous constituents of the urine. This theory explains why, during muscular exertion, the excretion of the nitrogenous constituents of urine is little or not at all increased, while that of carbonic acid is enormously augmented; for in a steam-engine, moderately fired and ready for use, the oxidation of iron, &c.

* For the last three years one of us has in his lectures brought forward this doctrine as an hypothesis, but he was not willing to present it to the public until he could bring undeniable facts to prove it.

would go on tolerably equably, and would not be much increased by the more rapid firing necessary for working, but much more coal would be burnt when it was at work than when it was standing idle. We therefore conclude that since the burning of albumen cannot be the only source of muscular power, it is not in any way concerned in its production; and to this conclusion we are impelled by the consideration that in so delicate an apparatus as a muscle-fibre must be, it is not likely that different kinds of chemical processes should be employed to produce the same effect. Even steam-engines are not indifferent as to the material burnt in them; in one made to burn wood, it would not do to use coal. How, then, is it possible to conceive that the muscle-machine was constructed especially for albumen, and that when enough albumen is not to be had, it puts up contentedly with non-nitrogenous fuel? That it does make use of non-nitrogenous material, we have by our experiment proved beyond a doubt. We therefore conclude that *since the muscle-machine can undoubtedly be heated by means of the non-nitrogenous fuel, this fuel is in all cases that best suited for it.*

In conclusion we may be permitted to return to the more general considerations touched upon in the opening of our paper. By the light of our hypothesis, the great efforts for the digestion of the hydrates of carbon which we meet with in the animal world are perfectly intelligible. We see, for instance, among the ruminants, most complicated apparatus, constructed for the purpose of saccharifying at least a little of the cellulose, hard as it is to dissolve, and of thus gaining something for the animal economy. This becomes immediately comprehensible on the assumption that hydrates of carbon subserve the most important functions of muscular work. These substances do not lose any of their importance as heat-producers in the ordinary sense of the phrase, because in muscular work a great part of the heat produced by oxidation is liberated as such, and because even the heat converted into work is always at last changed back into heat in the body of the animal; for it is only exceptionally that the animal is methodically employed by man for the production of external mechanical work.

There is another consideration connected with the preceding one, which is well calculated to make our conclusion appear *à priori* extremely probable. Among those animals whose muscles have enormous strength, there are several whose nourishment contains very little albumen, but, on the other hand, large quantities of the hydrates of carbon,—for instance, the swift ruminants, the goat, the chamois and gazelle, and many flying insects. Is it conceivable that the great muscular exertions of these creatures are really sustained by the oxidation of albumen?

We will here quote a remarkable fact bearing upon our theory, which was lately communicated to us by Dr. Piccard. The chamois-hunters of western Switzerland are accustomed, when starting on long and fatiguing expeditions, to take with them as provisions nothing but bacon-fat and sugar, because, as they say, these substances are more nourishing than meat. What they mean by this expression is, that they have learnt by experience that, in the form of fat and sugar, they can most conveniently carry with them a rich provision of force-producing oxidizable matter. With regard to this point, we can assert, from our own experience in the ascent of the Faulhorn, that in spite of the amount of work and the abstinence for thirty-one hours from albuminous food, we neither of us felt in the least exhausted. This could hardly have been the case if our muscular force had not been sustained by the non-nitrogenous food of which we partook.

LXXI. *On Mr. Cooke's Observations of the Solar Spectrum.*

By BALFOUR STEWART, M.A., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN Silliman's Journal for March 1866, there appears a communication from Mr. Josiah Cooke, Jun., Cambridge, U.S., "On the Aqueous Lines of the Solar Spectrum," which communication has been republished in the Philosophical Magazine of last month.

The author, who has kindly forwarded me a copy of his paper, alluded to his having visited Kew Observatory in the summer of 1864, when he was surprised to find my spectroscope less powerful than the one he was then using. He considers that "the facts stated in his paper fully account for the discrepancies in the representations which different observers have given of the D lines," and that "the moist climate of England is the evident explanation of the additional lines."

Mr. Balfour Stewart, the Superintendent of Kew Observatory, has forwarded me the following letter. You will observe that the observations of Mr. Stewart and of the other Kew observers are somewhat at variance with those of Mr. Cooke.

Believe me,

Yours truly,

Clapham Common, May 8, 1866.

J. P. GASSIOT.

Kew Observatory, May 7, 1866.

MY DEAR SIR,—It has occurred to me that perhaps you might like to know how far the observations made with your spectroscope at Kew corroborate the remarks of Mr. J. P. Cooke as to

the influence of aqueous vapour on that small but interesting region of the spectrum embraced by the two D lines.

The observations made at Kew, in which your most powerful arrangement of prisms (the sulphuret-of-carbon set) was used, may be considered comparable with any of the observations of Mr. Cooke. There may possibly be a small preponderance of power of the one instrument over the other, although I do not know which possesses it; but evidently, from the sketches given, these two arrangements are of the same order as regards power. The sulphuret-of-carbon prisms were in operation at Kew for about four months, until it was found that a glass train of inferior power was really more useful for your main object of obtaining an accurately-measured map of the solar spectrum; and during these four months the region between the two D lines was repeatedly observed, especially by Mr. Loewy.

These observations were made under varying atmospheric conditions, but in all these the same number of fine lines between the two broad D lines was invariably observed. The appearance presented by this region was that you have sketched in a paper communicated to the Royal Society on March 17, 1864, and it differs very little from fig. 4. given by Mr. Cooke in the *Philosophical Magazine* for May 1866, as representing an observation of this region made by him when the weight of vapour in one cubic foot of air was 6.57 grains.

When, however, we analyze the state of the atmosphere at Kew at the time (from midday to 3 P.M. of March 12, 1864) when the observation recorded and sketched by you was made, we find that the weight of vapour in one cubic foot of air was only 1.98 grain. In as far as we can judge of the vaporous condition of the whole atmosphere by a hygrometrical observation, the vapour of the atmosphere was much less at Kew at the time of this sketch than it was at the time when Mr. Cooke observed the similar appearance which he has represented in fig. 4; indeed the air at Kew at this moment probably contained less vapour than at the moment when he observed the appearance given in fig. 2, where there are only two lines between the lines D. But your sketch exhibits thirteen intervening lines.

Our Kew experience therefore does not quite accord with Mr. Cooke's results; for we have obtained—

(1) In all our observations, extending over the period of perhaps one month, the same number of lines between the two D lines.

(2) One of these observations was made at a time when there was probably little vapour in the atmosphere, and yet the spectrum obtained at Kew is similar to that obtained by Mr. Cooke at a time when there was much vapour.

Our experience is therefore against a decrease of vapour producing a decrease of lines in this region, but not perhaps against a decrease of vapour producing a diminution in the intensity of the group of lines near the most refrangible D line. This is the group that Mr. Cooke has found to vanish altogether when the amount of vapour is small. We have never found them to vanish, but we have observed a change of intensity, which may perhaps be due to vapour.

I shall only make one further remark. An observation of the spectrum of ignited sodium (made, I think, in the presence of Mr. Huggins) discovered the existence of at least two sodium lines between the two D lines. In this region, therefore, there are at least four lines due to sodium; and it might be natural to suppose that the absorption lines due to the presence of sodium vapour in the sun's atmosphere should be at least four in number for the same region. Besides these there is probably a nickel line; so that on these grounds I should hesitate in believing that Mr. Cooke's fig. 1, in which there are only three lines in all, namely the two D lines and one intervening line, is the ultimate representation of this region by a very powerful instrument.

I remain,

Yours very truly,

J. P. Gassiot, Esq., V.P.R.S.

B. STEWART.

LXXII. *On the Action of Carbonic Oxide on Sodium-ethyle.* By J. ALFRED WANKLYN, F.R.S.E., Professor of Chemistry at the London Institution*.

NEITHER carbonic acid nor carbonic oxide acts upon zinc-ethyle; but both of these gases attack sodium-ethyle. In the case of carbonic acid, the product of the reaction upon sodium-ethyle is, as I showed some years ago, propionate of soda. This reaction is very energetic, evolving much heat, and taking place without the application of external heat when the gas is simply passed over the sodium-compound.

The action of carbonic oxide is much less energetic. When the compound containing sodium-ethyle and zinc-ethyle, which I have described on a former occasion†, is sealed up with carbonic oxide, there is no perceptible change at first, but after a time a black deposit makes its appearance. If the apparatus be kept at ordinary temperatures, a considerable time is required for the production of this deposit; but if it be heated to temperatures approaching 100° C., then the blackening takes place immediately.

The black deposit is not carbonaceous, for it dissolves in hy-

* Communicated by the Author.

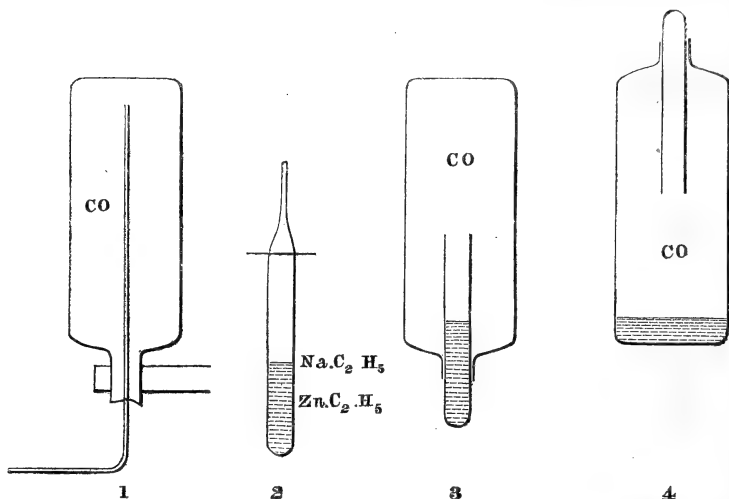
† *Ann. der Chem. und Pharm.* (1858), vol. cviii. p. 67.

drochloric acid. It is metallic, consisting of metallic zinc, and most probably also of metallic sodium. Absorption of the gas and formation of a fragrant oil accompany this deposit of metal.

The following is a description of the method adopted for the preparation of this oil in sufficient quantity to admit of an examination of it.

The carbonic oxide was prepared by Fownes's process, viz. by heating ferrocyanide of potassium with excess of sulphuric acid. It was collected in a large gas-holder, and after being passed through potash-lye and sulphuric acid, filled into Winchester quart bottles (stoppered bottles holding from $2\frac{1}{2}$ to 3 litres) by displacement (see fig. 1).

The sodium-ethyle was prepared in small tubes, each tube being charged with about 12 grms. of zinc-ethyle and 1 gm. of sodium, which was made to act very completely upon the zinc-ethyle by being shaken and gently warmed with it. The sodium-ethyle having been fully formed, the tube containing it had its top cut off (see fig. 2), and was then introduced into the bottle filled with carbonic oxide (see fig. 3). The apparatus was then reversed,



and the sodium-ethyle shaken out into the bottle containing the carbonic oxide. Lastly the bottle containing the carbonic oxide and the sodium-ethyle was stoppered and placed in warm water, and repeatedly shaken. After a short time the contents of the bottle became very black, and the digestion in warm water was stopped. The bottle was allowed to cool, then a little mercury and water poured in, and shaken up well. The aqueous solution was then introduced into a retort and distilled. Along

with the first few drops of the aqueous distillate an oil distilled over.

The amount of oil yielded by a Winchester quart of carbonic oxide and a charge of sodium-ethyle from 12 grms. of zinc-ethyle and 1 grm. of sodium was about 1 grm. The residue in the retort after the oil had been distilled off was examined and found to be very alkaline.

The product from a good many Winchester quarts of carbonic oxide was employed in the following experiments.

The oil, after being dried, was rectified and found to consist essentially of two portions—one boiling at 100° to 110° C., and the other at 150° , or a little higher. The portion with the lower boiling-point was freed from the rest by careful fractionation. Combustion of it gave these results:—

- I. .2076 grm. gave .5218 CO^2 and .2300 grm. $\text{H}^2 \text{O}$.
 II. .1029 grm. gave .2597 CO^2 and .1155 grm. $\text{H}^2 \text{O}$.

	I.	II.
Carbon	68.55	68.83
Hydrogen	12.31	12.47
Oxygen	19.14	18.70
	<u>100.00</u>	<u>100.00</u>

Deducing the formula by the well-known method, the following results are arrived at:—

$$\begin{aligned} \text{C} & . . . \quad 68.55 \div 12 = 5.71 \div 1.196 = 4.79 \\ \text{H} & . . . \quad 12.31 \div 1 = 12.31 \div 1.196 = 10.30 \\ \text{O} & . . . \quad 19.14 \div 16 = 1.196 \div 1.196 = 1.00 \end{aligned}$$

Therefore I. gives $\text{C}=4.79$; $\text{H}=10.30$; $\text{O}=1.00$.

$$\begin{aligned} \text{C} & . . . \quad 68.83 \div 12 = 5.736 \div 1.15 = 4.988 \\ \text{H} & . . . \quad 12.47 \div 1 = 12.47 \div 1.15 = 10.84 \\ \text{O} & . . . \quad 18.70 \div 16 = 1.150 \div 1.15 = 1.00 \end{aligned}$$

Therefore II. gives $\text{C}=4.988$; $\text{H}=10.84$; $\text{O}=1.00$.

From which it follows that the formula is $\text{C}^5 \text{H}^{10} \text{O}$, which requires

C^5	60	69.77
H^{10}	10	11.63
O	16	18.60
	<u>86</u>	<u>100.00</u>

It will be observed that both analyses point decisively to C^5 , but that, whilst I. requires H^{10} , II. gives a result approximating more nearly to H^{11} than H^{10} . The fact that the estimations of hydrogen are usually in excess, and that H^{11} would be in opposition to the law of saturation, fully justifies the selection of the formula $\text{C}^5 \text{H}^{10} \text{O}$. There is, moreover, as will presently appear, a special reason why the hydrogen should be in excess.

The resulting oil has therefore the composition of a compound containing two atoms of ethyle and one atom of carbonic oxide,



and all the facts observed are in harmony with the account of the reaction given by this equation,



There is absorption of gas and precipitation of metal. Obviously sodium liberated in presence of zinc-ethyle will act upon the zinc-ethyle, precipitating the zinc.

The oil having the composition $CO(C^2 H^5)^2$ appears to be identical with propione. It has the proper kind of smell, about the right boiling-point, and does not combine with bisulphite of soda, in this respect agreeing with the propione of Morley prepared from propionate of baryta, and the propione of Freund obtained by the action of zinc-ethyle on chloride of propionyle. Further reasons for concluding that it is identical with propione will be given directly.

The ketones are able to take up nascent hydrogen, giving secondary alcohols; it is therefore to be expected that the propione should have been partially converted into amylene-hydrate during the treatment of the mass of reduced metal and propione with mercury and water. This will fully account for the analyses exhibiting the hydrogen rather in excess over the formula required for propione. Further support is furnished by a peculiarity observed in the distillation of the substance. It commenced to distil with great precision at 99° and 100° , and at 110° the retort was quite dry; but the boiling-point had no tendency to remain constant at any point intermediate between these two extremes.

Now propione boils at 101° C., and amylene-hydrate at 108° C.

No attempt was made to effect a separation by fractional distillation.

With the view of further ascertaining the nature of the compound $CO(C^2 H^5)^2$, it was submitted to the oxidizing action of a mixture of bichromate of potash and dilute sulphuric acid. For this purpose a quantity of the product was prepared, and rectified ten times. It was also heated in a stream of dry carbonic acid gas, so as to expel all traces of ether, and then sealed up with excess of bichromate of potash and dilute sulphuric acid, and heated in the water-bath for many hours. The action takes place very slowly; and indeed one attempt to effect the oxidation in a retort utterly failed.

After the oil had disappeared, the digestion-tube was opened; there was no escape of gas, proving the non-formation of carbonic acid during the oxidation. The contents of the digestion-

tube had the well-known smell of the lower members of the fatty acid series, and after being diluted with water were distilled, and gave an acid distillate. This distillate was redistilled, and the distillate converted into a baryta-salt. The baryta-salt was carefully tested for formiates, and contained none.

It was dried at 110° C. and analyzed.

I. .3922 grm. of baryta-salt was ignited, and evolved abundance of organic matter, leaving .2804 grm. carbonate of baryta. Therefore Ba per cent. = 49.77.

II. .6266 grm. was precipitated with dilute sulphuric acid; and the resulting sulphate of baryta weighed .5295 grm. Therefore Ba per cent. = 49.74.

These numbers are intermediate between those required by propionate and acetate of baryta—a result which clearly shows that the liquid is broken up on oxidation, and, since neither carbonic acid nor formic acid is produced, indicates pretty clearly that the results of the oxidation are propionic and acetic acids.

Confirmation of this fact was obtained by applying Liebig's method of fractional saturation to the mixed acid. Some of the acid was partially saturated with carbonate of soda and distilled, and the distillate made into a baryta-salt.

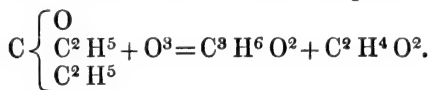
.2066 grm. of this baryta-salt gave .1728 grm. of sulphate of baryta. \therefore Ba per cent. = 49.24.

Propionate of baryta contains 48.48 per. cent. of barium. If higher acids than propionic had been there, an utterly different result would have been obtained.

The residue in the retort was then supersaturated with sulphuric acid and distilled, when it gave a distillate which presented all the characters of acetic acid.

It is therefore established that the oxidation-products are propionic and acetic acids, and that neither carbonic acid nor formic acid is given.

Bearing in mind the origin of the compound, this is a very interesting result. Formed by the union of carbonic oxide with ethyle, it gives the characteristic oxidation-product of the propyle series, and the characteristic oxidation-product of the ethyle series,



According to theory, normal propione should fall into the groups C^3 and C^2 when oxidized; this reaction therefore indicates that the product obtained from carbonic oxide and sodium-ethyle is identical with normal propione.

In conclusion, I cannot refrain from referring to Kolbe's speculations on the nature of the ketones. Many years ago he

described these compounds as consisting of carbonic oxide in union with the alcohol-radicals. In the reaction which forms the subject of this paper, carbonic oxide is seen to unite with the alcohol-radicals and to form a genuine ketone: a more remarkable confirmation of the theory is hardly conceivable.

LXXIII. *On Aqueous Vapour and Terrestrial Radiation.*

By M. G. NEUMAYER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I TRANSMIT to you a communication from M. Neumayer, which I trust will have the effect of stimulating other meteorologists to enter upon and pursue the line of inquiry which he has so ably and zealously begun. There is, in my estimation, no result of physical science more certain than that the aqueous vapour of our air exerts a powerful influence upon the radiation from our earth; and there is none, I imagine, more likely to guide the really scientific meteorologist to results of permanent value.

JOHN TYNDALL.

To Professor Tyndall, F.R.S., &c.

Frankenthal, Pfalz a. Rhein,

May 4, 1866.

MY DEAR SIR,

You will perhaps remember that some two years ago I sent you some original observations made at the Flagstaff Observatory, Melbourne, bearing on the absorption of heat by aqueous vapour, and its relation to terrestrial radiation. At the time when I did myself the pleasure to call upon you at London, you stated that great pressure of other matters did not admit of your undertaking the task of discussing those observations; and in consequence of this I had the extensive labour of classifying and condensing so large a number of figures done in my bureau of computation, established with the object of publishing discussions on the magnetic and meteorological observations made by me during the years 1858-63. In the annexed Tables, four in number, you receive the results of my labours in this direction, which I feel great pleasure in putting before you.

In order that you may be able to form an opinion as to the manner in which the observations were made, I subjoin the following remarks.

(1) The observations were made at all times of the day and the night whenever the sky was clear, *i. e.* the zenith perfectly free from clouds.

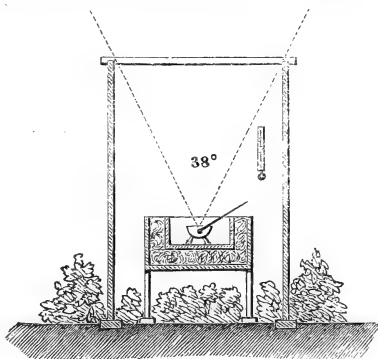
(2) The tension of aqueous vapour and the degree of relative humidity were observed by August's psychrometer, continually checked, however, by Döbereiner-Regnault's aspirator hygro-

meter. The reductions were effected by Regnault's Tables, the thermometers being Kew standards.

(3) The radiation-instrument consisted of a spirit minimum-thermometer of Kew, the bulb of which was placed and carefully adjusted in the focus of a parabolic reflector nicely polished and silvered, 6·4 inches wide and 2·4 inches deep*. This reflector was put in a box filled with cotton and placed in a little house, keeping out the rays of the sun, but in such a manner that the zenith over the instrument remained perfectly free for a space of some 38° . The focus of the reflector was about $1\frac{1}{2}$ feet above the surrounding ground (see introduction to 'Results,' page 4.)

(4) The psychrometer was placed close to this radiation-apparatus; and in order to have a control, a thermometer was placed within the little house (see annexed figure), the same being frequently compared with the Kew standard.

(5) Table I. contains in its first column the number of single simultaneous observations, the second column shows the mean pressure of aqueous vapour, the next the mean temperature of the air, and the last the mean difference between temperature of air and radiation, obtained by the instruments described. The results have been arranged according to the value of the aqueous tension.



(6) Table II. shows the relation between the degree of relative humidity of air and the radiation-difference. For every single set of observations the relative humidity was computed.

(7) It remains yet to be mentioned that the elaborate task of classifying and reducing the 4376 single sets of observations has been twice executed, both computations being quite independent of each other.

(8) With a view to show that the decrease in radiation with an increase in humidity, as evident from Table II., is not due to the decrease in temperature, as may be suspected when glancing over Table II., two more Tables were constructed illustrative of the influence the temperature of the air alone exerts on the radiation-instrument (Tables III. and IV.). For, although fully aware of the principles generally adopted with regard to the influence of the temperature of the air on radiation, and also of the results of your investigations (§ XII. p. 406), to the

* In strict conformity with the instructions of the Royal Society.

effect that the temperature of the surrounding air does not sensibly influence the difference of temperature between the radiating body and the temperature of the air, I thought one might be inclined to suspect undue radiation influencing the reading of the parabolic thermometer, and I took for this reason the trouble of classifying the observations merely according to temperature of the air.

The results at which I arrive may be summed up in the following lines:—

(a) The absolute quantity of aqueous vapour in the air is in itself alone not sufficient as a criterion for the degree of radiation; or if it should be otherwise, the instrument used for the observations is not sufficiently delicate to show the degree of influence.

(b) The absolute quantity of aqueous vapour, together with a certain temperature, *i. e.* the relative humidity of air, greatly influence terrestrial radiation, in such a manner that, the greater the degree of relative humidity, the less radiation is noticeable (see Table II.). With respect to the latter remark, I cannot refrain from adding a few words. The law is even noticeable, though less distinctly expressed, in the lower per cents. of humidity in extreme cases of dryness—although in all these cases a thin veil of cirrostratus clouds covers the sky, so delicate as frequently not to be perceptible by the eye. It is further to be remarked that, during hot winds, when the air is extremely dry, quantities of dust are floating in the air, seriously interfering with radiation, although the sky is apparently quite clear. All these cases have been included in deriving the above results; and considering all, we must be surprised that, notwithstanding all this, an increase of radiation of between 15 and 30 per cent is perceptible.*

It is scarcely necessary for me to dwell on the apparent connexion between the increase of temperature of the air and the amount of radiation shown by Table II., such being fully accounted for by the relation between temperature and relative humidity. Tables III. and IV. sufficiently prove that the radiation-instrument was not influenced by the surrounding air in any undue manner so as to interfere with the recognition of the general law.

I hastened to put these facts before you, in order you might be able to convince yourself of the reliability of the observations and the method of discussion; and further, because I was convinced you would find it important to receive proofs that by the aid of meteorological instruments generally in use we are able to recognize a function of aqueous vapour in our atmosphere which

* I have shown that the smoke of the London air is feeble in comparison with its aqueous vapour.—J. T.

hitherto had only been demonstrated by your own important experiments (as far as my knowledge on this point goes).

You would oblige me exceedingly by giving me your ideas on the various points raised in this letter. According to your opinion, I purpose either turning the results to account by publishing them in my next publication, or hand them over to you with no other condition but that I shall be supplied with whatever you shall write upon this special subject.

Requesting you kindly to excuse the liberty I have taken in thus troubling you, I remain, my dear Sir,

Yours very truly,

TABLE I.

G. NEUMAYER.

No. of obs.	Mean			No. of obs.	Mean			No. of obs.	Mean		
	Pressure of vapour.	Temp. of air.	Temp. of air minus temp. radia- tion.		Pressure of vapour.	Temp. of air.	Temp. of air minus temp. radia- tion.		Pressure of vapour.	Temp. of air.	Temp. of air minus temp. radia- tion.
	inch.				inch.				inch.		
4	0.1712	32.1	1.35	29	0.2703	55.1	3.56	45	0.3483	64.8	4.03
43	.1929	37.7	2.73	44	.2704	50.1	3.82	44	.3485	56.3	3.37
10	.1956	46.9	3.38	44	.2869	54.7	3.74	48	.3493	65.0	3.17
42	.2100	39.9	2.62	41	.2878	56.4	3.73	45	.3497	66.9	3.93
34	.2114	45.3	2.88	43	.2879	54.0	4.31	49	.3677	58.7	3.24
43	.2120	46.4	2.88	41	.2884	56.8	4.00	45	.3680	70.2	3.55
44	.2133	49.5	3.52	43	.2886	52.7	3.39	44	.3687	64.5	3.72
45	.2293	43.2	2.98	43	.2888	52.5	3.85	48	.3687	63.4	3.23
40	.2302	43.6	3.34	45	.2892	59.6	3.56	24	.3692	63.0	3.69
40	.2304	46.2	3.10	40	.2894	57.2	4.00	44	.3692	64.4	4.34
43	.2307	45.4	3.23	45	.2899	50.7	3.13	43	.3701	60.5	3.52
43	.2307	51.3	4.60	40	.2900	56.5	3.91	44	.3876	66.3	4.50
44	.2313	52.9	2.69	49	.2915	53.3	3.97	49	.3888	61.8	3.60
29	.2322	52.6	3.51	45	.2940	61.9	3.70	43	.3894	63.2	3.76
44	.2484	47.3	3.37	44	.3076	53.0	3.21	45	.3900	69.2	3.77
44	.2493	46.5	2.81	40	.3080	61.1	4.58	19	.3909	59.0	3.32
43	.2494	47.1	3.32	40	.3088	57.5	3.38	44	.4081	65.9	3.84
41	.2496	52.6	3.41	43	.3091	57.6	3.95	49	.4087	63.5	3.60
44	.2497	48.2	2.32	43	.3093	58.1	4.10	43	.4092	62.3	4.00
43	.2497	49.4	3.60	44	.3095	54.2	3.83	3	.4100	64.4	3.07
43	.2502	54.9	3.60	24	.3095	55.9	4.18	48	.4280	68.1	4.24
45	.2504	46.3	4.52	44	.3096	55.0	3.95	9	.4290	64.0	3.29
40	.2510	59.8	4.12	45	.3097	64.4	3.47	49	.4295	67.1	4.13
40	.2512	48.2	3.09	44	.3111	57.7	3.82	32	.4360	67.5	3.56
6	.2533	54.9	3.10	4	.3232	55.1	3.30	35	.4476	66.7	3.77
41	.2523	52.1	3.53	44	.3280	57.7	4.03	48	.4497	69.2	4.25
43	.2683	48.5	3.51	48	.3286	55.5	3.82	48	.4692	70.6	3.66
45	.2687	52.7	3.71	43	.3287	59.1	3.80	26	.4698	71.7	3.84
44	.2687	50.2	3.60	41	.3290	59.4	3.90	3	.4886	68.7	2.86
40	.2688	56.3	4.05	44	.3291	55.2	3.90	48	.4893	73.6	3.04
44	.2689	50.2	3.58	45	.3294	59.3	3.64	48	.5096	73.1	3.47
41	.2691	56.4	3.74	40	.3298	66.7	4.33	27	.5284	76.8	3.58
44	.2692	47.7	3.58	40	.3320	68.8	3.68	12	.5460	77.0	4.55
43	.2694	50.3	3.98	43	.3468	62.1	3.85	12	.5715	79.4	3.50
43	.2695	51.8	2.55	49	.3472	56.3	3.48	19	.5908	77.9	3.72
40	.2696	64.8	4.10	44	.3478	60.0	4.23	13	.6061	72.5	2.58
43	.2699	53.2	3.54	34	.3481	61.0	4.41	4	.6292	78.3	2.23

TABLE II.

Groups.	Relative humidity.	Number of observations.	Temperature of air.	Temperature of air minus radiation.
	per cent.			
1	22·417	190	84°·83	4°·79
2	40·220	406	71·43	4·69
3	47·479	244	68·03	4·43
4	52·455	286	66·46	4·37
5	57·521	322	61·99	3·92
6	62·580	301	59·70	3·70
7	67·441	322	58·01	3·71
8	72·485	357	55·12	3·52
9	77·427	438	52·67	3·48
10	82·598	458	51·43	3·36
11	87·397	426	49·63	3·21
12	92·348	403	46·26	3·11
13	96·643	223	43·33	2·55

TABLE III.

Number of		Mean pressure of vapour.	Mean temperature of air.	Temperature of air minus radiation.
Observations.	Groups.			
		inch.		
220	7	0·2009	42°·54	2°·77
284	7	·2307	47·89	3·35
259	6	·2493	48·52	3·14
215	6	·2514	52·70	3·57
257	6	·2687	52·38	3·70
213	7	·2698	53·30	3·59
385	9	·2885	54·95	3·74
258	6	·3000	57·22	3·79
243	6	·3000	57·53	3·91
268	7	·3253	57·10	3·64
295	7	·3401	62·03	3·95
276	6	·3552	63·65	3·55
296	7	·3746	63·40	3·80
384	11	·4117	64·93	3·69
343	13	·5229	73·5	3·46

In this Table are represented the means of the groups, irrespective of the number of single observations.

TABLE IV.

Number of observations.	Temperature of air.	Pressure of vapour.	Temperature of air minus radiation.
251	41°·46	0·2108	2°·96
747	48·30	0·2502	3·39
881	53·70	0·2799	3·58
919	58·01	0·3163	3·73
740	63·40	0·3628	3·76
522	68·15	0·4081	3·86
135	72·72	0·5187	3·23
74	77·88	0·5732	3·52

N.B. I need scarcely mention that the values arrived at only possess a relative worth with respect to the instruments used. An apparatus measuring terrestrial radiation absolutely and in a manner *which renders it practical*, is, I think, yet to be contrived.

LXXIV. *On the Spectra of some of the Fixed Stars.* By WILLIAM HUGGINS, F.R.A.S., and W. A. MILLER, M.D., LL.D., Treas. and V.P.R.S., Professor of Chemistry, King's College, London.

[Concluded from p. 425.]

§ V. *General Observations.*

20. **O**N the Colours of the Stars.—From the earliest ages it has been remarked that certain of the stars, instead of appearing to be white, shine with special tints; and in countries where the atmosphere is less humid and hazy than our own, this contrast in the colour of the light of the stars is said to be much more striking. Various explanations of the contrast of colours, by Sestini and others, founded chiefly on the difference of the wavelengths corresponding to the different colours, have been attempted, but as yet without success. Probably in the constitution of the stars as revealed by spectrum analysis, we shall find the origin of the differences in the colour of stellar light*.

Since spectrum analysis shows that certain of the laws of terrestrial physics prevail in the sun and stars, there can be little doubt that the immediate source of solar and stellar light must be solid or liquid matter maintained in an intensely incandescent state, the result of an exceedingly high temperature. For it is from such a source alone that we can produce light even in a feeble degree comparable with that of the sun.

The light from incandescent solid and liquid bodies affords an

* In connexion with this subject we quote the following passage from Smith's *Speculum Hartwellianum*, 4to, 1860, p. 315:—"Sir David Brewster observes that there can be no doubt that in the spectrum of every coloured star certain rays are wanting which exist in the solar spectrum; but we have no reason to believe that these defective rays are absorbed by any atmosphere through which they pass. And in recording the only observation perhaps yet made to analyze the light of the coloured stars, he says, 'In the orange-coloured star of the double star ζ Herculis, I have observed that there are several defective bands. By applying a fine rock-salt prism, with the largest possible refracting angle, to this orange star, as seen in Sir James South's great achromatic refractor, its spectrum had the annexed appearance [in the Campden Hill Journal], clearly showing that there was one defective band in the red space, and two or more in the blue space. Hence the colour of the star was orange, because there was a greater defect of blue than of red rays.' "

unbroken spectrum containing rays of light of every refrangibility within the portion of the spectrum which is visible. As this condition of the light is connected with the state of solidity or liquidity, and not with the *chemical* nature of the body, it is highly probable that the light when first emitted from the photosphere, or light-giving surface of the sun and of the stars, would be in all cases identical.

The source of the difference of colour, therefore, is to be sought in the difference of the constituents of the investing atmospheres*. The atmosphere of each star must vary in nature as the constituents of the star vary; and observation has shown that the stars do differ from the sun and from each other in respect of the elements of which they consist. The light of each star therefore will be diminished by the loss of those rays which correspond in refrangibility to the bright lines which the constituents of each atmosphere would, in the incandescent state, be capable of emitting. In proportion as these dark lines preponderate in particular parts of the spectrum, so will the colours in which they occur be weaker; and consequently the colours of other refrangibilities will predominate.

Of this the spectrum of α Orionis affords a good example. The green and blue parts of the spectrum are comparatively dark, from the numerous and close groups of dark lines. In the orange they are less strong. Hence it might be anticipated that the light of the star would be characterized by "an orange tinge," as noted by Smyth. β Pegasi is described by Smyth as "deep yellow;" and the appearance exhibited by its spectrum, which closely resembles that of α Orionis, though much fainter, supports the same view.

Aldebaran is recorded by Smyth as of a "pale rose tint." In the spectrum of this star, with the exception of the hydrogen line C, there are but few strong lines in the red, whilst the orange portion is considerably subdued by dark lines, which are less numerous in the green and blue. Sirius, on the contrary, is "brilliant white" (Smyth); and the continuous brightness of the spectrum, with the exception of five strong lines, is, as compared with Aldebaran and α Orionis, unaffected by the dark lines which cross it. The spectrum is indeed crowded with numerous fine lines; but the intensity of these lines is extremely feeble as

* The presence in the atmospheres of Aldebaran and α Orionis of metals, such as iron, which require an exceedingly high temperature to convert them into vapour, renders untenable the supposition, which might otherwise have been entertained, that the orange and red tints of the light of these stars might be due to an inferior degree of incandescence of the photosphere as compared with the temperature of the stars the light of which is white.

contrasted with those of the stars just mentioned. It may be that the length of the stellar atmosphere through which the light passes is less, relatively to the intensity of radiation from the photosphere, and so is insufficient to produce lines of the same degree of blackness as would be produced if the atmosphere were denser. The great intensity, however, of the light of Sirius would rather lead to the conclusion that the atmosphere of vapours is itself highly incandescent. If so, might it not to some extent replace with its own light the light which it has absorbed from the photosphere behind it? It matters little, however, for the present purpose, whether or not either of these suppositions be adopted. There is at all events a most striking difference between the effect on the colour of the star of the closely grouped and very dark lines in the green and blue portions of the spectrum of α Orionis and of the corresponding portion of the spectrum of Sirius, in which the dark lines are faint and wholly unequal to produce any noticeable subduing of the blue and green rays.

We have not yet had an opportunity of testing by experiment whether this hypothesis of the origin of the colours of the light of the stars is also applicable to the remarkable exceptional class of stars the light of which is of a decided green, blue, or violet colour. Such stars are usually very small, and they are always so closely approximated to other more brilliant stars, that it is scarcely possible, with the apparatus which we employ, to obtain separate images of the two spectra: and even were such separation easily practicable, the light of the strongly-coloured star is usually so feeble that its satisfactory prismatic analysis would be a matter of great difficulty.

[One of the objects proposed in the construction of the spectrum apparatus with which the additional observations on Jupiter, Saturn, and Mars were made, and which has been described (p. 415) in connexion with those observations, was to make it available for the prismatic observation of some double and multiple stars.

Before commencing the observation of the spectra of the components of a double star, it is necessary that the position-angle of the stars should be approximatively known. The spectrum apparatus has then to be rotated upon the end of the telescope until the direction of the slit becomes perpendicular to a line joining the stars. When the instrument is in this position, the images of the stars are elongated by the cylindrical lens into two short lines of light parallel with the slit, and separated from each other by a small interval. If the telescope be now moved in a direction at right angles to that of the slit, either of the elongated stellar images can, at pleasure, be made to fall upon the slit and form its spectrum in the instrument. By adopting this method of ob-

servation, the spectra of the components of β Cygni were separately examined. These spectra, especially that of B, are so faint that the lines are seen with difficulty, and scarcely admit of being measured. Since, however, on account of the strongly-contrasted colours of these stars, considerable interest attaches to a comparative examination of their spectra, we have represented in fig. 4, Plate V., the appearances which these spectra present to the eye, though we have not yet measured the lines and bands in them. These figures must be regarded as eye estimations only of the general features of the two spectra. The spectra contain, doubtless, many other lines; and the positions of the lines inserted in the drawings, with the exception of *b* and D, were not measured, but only roughly estimated. The distinctive characteristics of these spectra are in accordance with the theory of the origin of the colours of the stars proposed in the foregoing paragraphs. In the case of both stars, the portions of the spectrum which correspond to the colours which are deficient in the light of the star, are those which are most strongly shaded with bands of absorption. Thus in the spectrum of A, the light of which is yellow tinted with orange, the absorption is greatest in the violet and blue; for the strong lines in the orange and red, since they are narrow, would diminish in a much smaller degree the light of these refrangibilities. The yellow and part of the green are free from *strong* lines.

The light of the star B appears to us to be blue, though in some states of the atmosphere the star becomes greenish blue, green, and even greenish white. These changes are probably due to the comparatively greater absorptive action of the vapours in the air upon the more refrangible portions of the spectrum; in proportion to which absorption the other parts of the spectrum become relatively exalted, and thus predominate more or less in the eye.

This inequality of the absorptive action of the vapours of the atmosphere upon different parts of the spectrum becomes very evident if the eyepiece of the telescope be put out of focus (outside of the focus) so as to bring the blue and red rays to a focus in the centre of an expanded image of the star. In the case of B of β Cygni, the centre appears purple, surrounded with a margin of green. In proportion to the changes in the atmosphere, by the passage of masses of vapour or thin cloud, will be the variations of these colours. The green becomes greener; but the blue and the violet are affected in a much greater degree, at times fading almost completely; then the colours resume their former tints and brightness. Several such changes may sometimes occur during one observation.

The spectrum B, observed under conditions of atmosphere in

which the colour of the star was blue, was remarkable for the faintness of the orange and yellow portions, compared with the rest of the spectrum. The diminished brightness of these parts appears to be produced by several groups of closely set fine lines, while towards the more refrangible limit of the spectrum a few strong lines separated by considerable intervals are seen.

The observation of this star, on account of the faintness of its spectrum, is so difficult and fatiguing to the eye, that we have not been able to examine it more accurately or in greater detail.

We have by the same method of observation examined the spectra of the components of α Herculis. The spectrum of A is remarkable for the great strength of the groups of lines in the green, blue, and violet; fainter bands are visible in the yellow and orange, also two strong bands in the red. This arrangement of the bands of absorption agrees with the orange colour which strongly predominates in the light of this star.

B is bluish green in colour. The more refrangible portions of its spectrum are very bright in consequence of the absence of any strong bands. The yellow and the orange parts are crossed by several groups of lines.—August 31, 1864.]

The suggestive fact that stars of these more highly refrangible colours are always observed in close contiguity with much brighter stars, generally of an orange or red tint, would afford countenance to the supposition that these exceptional colours are due to some special physical conditions essentially connected with the stellar systems of which they seem to form a part.

Arago* remarks, "Among the sixty or eighty thousand *isolated* stars, the positions of which are to be found in the catalogues of astronomers, there are none, I think, inscribed with any other indications in regard to colour, than white, red, and yellow. The physical conditions which determine the emission of blue and green light appear, then, to exist only in *multiple* stars."

These stars are without exception feeble in the intensity of their light. The explanation is not admissible, that the faint blue or violet light is due to a less intense incandescence of the radiating surface, since it is precisely these more refrangible rays which would be the first to fail as the temperature diminished, and upon this supposition the star should be dull red. It is of course to be supposed that in the process of gradual cooling some bodies which are less volatile than others would cease to exist in the atmosphere at an earlier period than others, or that they might enter into new combinations more readily than others, and so modify the tint of the light emitted.

The existence around these blue stars of an extended atmo-

* Popular Astronomy, translated by Smith and Grant, vol. i. p. 295.

sphere of "fog" will not explain the absorption of the *less* refrangible portion of the luminous spectrum.

21. These spectrum observations are not without interest also when viewed in connexion with the *nebular hypothesis* of the cosmical origin of the solar system and fixed stars. For if it be supposed that all the countless suns which are distributed through space, or at least those of them which are bright to us, were once existing in the condition of nebulous matter, it is obvious that, though certain constituents may have been diffused throughout its mass, yet the composition of the nebulous material must have differed at different points; otherwise, during the act of agglomeration, each system must have collected and condensed equal proportions of similar materials from the mass around. It cannot be supposed that similarity in physical properties has caused the association of the different elements: we find, for example, some of the least volatile of the metals, such as iron, associated with highly volatile elements, such as mercury and tellurium, in the same star.

If we may so say, there seems to be some analogy between this irregular distribution of the elements in different centres in space, and the manner in which the components of the earth's crust are distributed. Upon the earth there are certain very generally diffused elements, such as oxygen, hydrogen, carbon, silicon, iron, aluminium, and calcium, which occur in all parts; whilst there are others which, like silver, tin, lead, and other metals, are accumulated at particular points only. Whatever may have been the physical causes which may have produced this separation, we see abundant evidence of the advantage of this distribution in their application to the purposes of man—smallness in relative amount being compensated for by the accumulation of the material in denser deposits, which allow of their comparatively easy extraction to supply the wants of mankind. If this arrangement be admitted as designed in the case of the earth, is it going beyond the limits of fair deduction to suppose that, were we acquainted with the economy of those distant globes, an equally obvious purpose might be assigned for the differences in composition which they exhibit?

22. The additional knowledge which these spectrum observations give us of the nature and of the structure of the fixed stars, seems to furnish a basis for some legitimate speculation in reference to the great plan of the visible universe, and to the special object and design of those numerous and immensely distant orbs of light.

The closely marked connexion, in similarity of plan and mode of operation, in those parts of the universe which lie within the range of experiment, and so of our more immediate knowledge,

renders it not presumptuous to attempt to apply the process of reasoning from analogy to those parts of the universe which are more distant from us.

Upon the earth we find that the innumerable individual requirements which are connected with the present state of terrestrial activity, are not met by a plan of operation distinct for each, but are effected in connexion with the special modifications of a general method embracing a wide range of analogous phenomena. If we examine living beings, the persistence of unity of plan observable amidst the multiform varieties of special adaptation of the vertebrate form of life may be cited as an example of the unity of operation referred to. In like manner the remarkably wide range of phenomena which are shown to be reciprocally interdependent and correlative of each other, by the recent great extension of our knowledge in reference to the relation of the different varieties of force and their connexion with molecular motion, exhibits a similar unity of operation amidst the changes of the bodies which have not life.

The observations recorded in this paper seem to afford some proof that a similar unity of operation extends through the universe as far as light enables us to have cognizance of material objects. For we may infer that the stars, while differing the one from the other in the kinds of matter of which they consist, are all constructed upon the same plan as our sun, and are composed of matter identical, at least in part, with the materials of our system.

The differences which exist between the stars are of the *lower order*, of differences of *particular adaptation*, or special modification, and not differences of the *higher order* of distinct *plans of structure*.

There is therefore a probability that these stars, which are analogous to our sun in structure, fulfil an analogous purpose, and are, like our sun, surrounded by planets, which they by their attraction uphold, and by their radiation illuminate and energize. And if matter identical with that upon the earth exists in the stars, the same matter would also probably be present in the planets genetically connected with them, as is the case in our solar system.

It is remarkable that the elements most widely diffused through the host of stars are some of those most closely connected with the constitution of the living organisms of our globe, including hydrogen, sodium, magnesium, and iron. Of oxygen and nitrogen we could scarcely hope to have any decisive indications, since these bodies have spectra of different orders. These forms of elementary matter, when influenced by heat, light, and chemical force, all of which we have certain knowledge are radiated from the stars, afford some of the most important conditions

which we know to be indispensable to the existence of living organisms such as those with which we are acquainted. On the whole we believe that the foregoing spectrum observations on the stars contribute something towards an experimental basis on which a conclusion, hitherto but a pure speculation, may rest, viz. that at least the brighter stars are, like our sun, upholding and energizing centres of systems of worlds adapted to be the abode of living beings.

TABLE of Stellar Spectra.

Aldebaran.				α Orionis.				β Pegasi.			
822.5	H	1107		840		1139.5		896			
855.5		1112	Te	860		1144		923			
872.5		1117.5	Sb	870		1145.5		1000	} Na		
880		1143 <i>d</i>		881		1148		1002			
893.5		1158		887		1151		1014			
900		1164	Hg	890		1158.5		1165			
903.5		1171.5		899		1167		1220			
907.5		1178		911		1169.5		1276.5			
915		1187		918	Ca	1176.5		1291.5	} Mg		
918	Ca	1192		920		1183.5		1297.5			
923	Hg	1202		929		1187		1300.5			
933	Ca	1210		933	Ca	1191.5		1350.5			
945.5	Sb	1224.5		936		1198		1392.5			
951.5		1240		946		1201.5		1425			
954.5		1241.5		966		1210	Te	1515			
956		1250		968.5		1214		1732			
966.5	Sb	1252	Fe	976		1220.5		1835			
972.5		1269.5	Fe	983		1225					
976	Te	1272		992		1237					
982		1277	Bi	1000	} Na	1243					
986.5		1282		1002		1252	Fe				
993		1291.5		1010.5		1262					
1000	} Na	1297.5	} Mg	1013	Ca	1269.5	Fe				
1002		1300.5		1030		1277	Bi				
1013	Ca	1314	Bi	1040		1280.5					
1023		1323		1050.5		1285.5					
1028		1328		1062	Bi	1291.5					
1031		1351		1069.5		1297.5	} Mg				
1036.5	Hg	1420	Fe	1079.5		1300.5					
1040		1442.5	Fe	1085.5		1303					
1044	Hg	1483	H	1090		1314	Bi				
1058				1091.5		1334					
1062	Bi			1099		1350					
1067	Te			1105	Ca	1356					
1076				1109.5		1361					
1086.5	Te			1116.5		1416					
1095				1123.5		1420	Fe				
1100				1132		1442.5	Fe				
1105	Ca			1135.5		1557					

LXXV. *On the Spectra of some of the Nebulæ.* By WILLIAM HUGGINS, F.R.A.S. *A Supplement to the Paper "On the Spectra of some of the Fixed Stars."* By WILLIAM HUGGINS, F.R.A.S., and W. A. MILLER, M.D., LL D., Treas. and V.P.R.S.*

[With a Plate.]

THE concluding paragraphs of the preceding paper refer to the similarity of essential constitution which our examination of the spectra of the fixed stars has shown in all cases to exist among the stars, and between them and our sun.

It became therefore an object of great importance, in reference to our knowledge of the visible universe, to ascertain whether this similarity of plan observable among the stars, and uniting them with our sun into one great group, extended to the distinct and remarkable class of bodies known as nebulæ. Prismatic analysis, if it could be successfully applied to objects so faint, seemed to be a method of observation specially suitable for determining whether any essential physical distinction separates the nebulæ from the stars, either in the nature of the matter of which they are composed, or in the conditions under which they exist as sources of light. The importance of bringing analysis by the prism to bear upon the nebulæ is seen to be greater by the consideration that increase of optical power alone would probably fail to give the desired information; for, as the important researches of Lord Rosse have shown, at the same time that the number of the clusters may be increased by the resolution of supposed nebulæ, other nebulous objects are revealed, and fantastic wisps and diffuse patches of light are seen, which it would be assumption to regard as due in all cases to the united glare of suns still more remote.

Some of the most enigmatical of these wondrous objects are those which present in the telescope small round or slightly oval disks. For this reason they were placed by Sir William Herschel in a class by themselves under the name of Planetary Nebulæ. They present but little indication of resolvability. The colour of their light, which in the case of several is blue tinted with green, is remarkable, since this is a colour extremely rare amongst single stars. These nebulæ, too, agree in showing no indication of central condensation. By these appearances the planetary nebulæ are specially marked as objects which probably present phenomena of an order altogether different from those which characterize the sun and the fixed stars. On this account, as well as because of their brightness, I selected these nebulæ as the most suitable for examination with the prism.

* From the Philosophical Transactions for 1864, Part II. Communicated, with additional notes, by the Author.

The apparatus employed was that of which a description was given at page 415. A second eyepiece was used in these observations, having a magnifying-power of nine diameters. For the greater part of the following observations on the nebulae, the cylindrical lens is not necessary, and was removed from the instrument. The numbers and descriptions of the nebulae, and their places for the epoch 1860, January 0; included within brackets, are taken from the last Catalogue of Sir John Herschel*.

[No. 4373. 37 H. IV. R.A. $17^h 58^m 20^s$. N.P.D. $23^\circ 22' 9'' \cdot 5$. A planetary nebula; very bright; pretty small; suddenly brighter in the middle, very small nucleus.] In Draco.

On August 29, 1864, I directed the telescope armed with the spectrum-apparatus to this nebula. At first I suspected some derangement of the instrument had taken place; for no spectrum was seen, but only a short line of light perpendicular to the direction of dispersion. I then found that the light of this nebula, unlike any other ex-terrestrial light which had yet been subjected by me to prismatic analysis, was not composed of light of different refrangibilities, and therefore could not form a spectrum. A great part of the light from this nebula is monochromatic, and after passing through the prisms remains concentrated in a bright line occupying in the instrument the position of that part of the spectrum to which its light corresponds in refrangibility. A more careful examination with a narrower slit, however, showed that, a little more refrangible than the bright line, and separated from it by a dark interval, a narrower and much fainter line occurs. Beyond this, again, at about three times the distance of the second line, a third exceedingly faint line was seen. The positions of these lines in the spectrum were determined by a simultaneous comparison of them in the instrument with the spectrum of the induction spark taken between electrodes of magnesium. The strongest line coincides in position with the brightest of the air lines. This line is due to nitrogen, and occurs in the spectrum about midway between *b* and *F* of the solar spectrum. Its position is seen in Plate VI.†

The faintest of the lines of the nebula agrees in position with the line of hydrogen corresponding to Fraunhofer's *F*. The other bright line was compared with the strong line of barium 2075‡: this line is a little more refrangible than that belonging to the nebula.

Besides these lines, an exceedingly faint spectrum was just perceived for a short distance on both sides of the group of bright

* Philosophical Transactions, Part I. 1864, pp. 1-137.

† See also Philosophical Transactions, 1864, p. 156, and plate 1.

‡ Ibid. p. 156.

lines. I suspect this is not uniform, but is crossed with dark spaces. Subsequent observations on other nebulæ induce me to regard this faint spectrum as due to the solid or liquid matter of the nucleus, and as quite distinct from the bright lines into which nearly the whole of the light from the nebula is concentrated.

In the diagram (fig. 5, Plate V.) the three principal lines only are inserted, for it would be scarcely possible to represent the faint spectrum without greatly exaggerating its intensity.

The colour of this nebula is greenish blue.

[No. 4390. 2000 h. Σ 6. R.A. $18^h 5^m 17^s.8$. N.P.D. $83^\circ 10' 53''.5$. A planetary nebula; very bright; very small; round; little hazy.] In Taurus Poniatowskii.

The spectrum is essentially the same as that of No. 4373.

The three bright lines occupy the same positions in the spectrum, which was determined by direct comparison with the spectrum of the induction-spark. These lines have also the same relative intensity. They are exceedingly sharp and well defined. The presence of an extremely faint spectrum was suspected. In connexion with this it is important to remark that this nebula does not possess a distinct nucleus.

The colour of this nebula is greenish blue.

[No. 4514. 2050 h. 73 H. IV. R.A. $19^h 41^m 7^s.5$. N.P.D. $39^\circ 49' 41''.7$. A planetary nebula with a central star. Bright; pretty large; round; star of the 11th magnitude in the middle.] In Cygnus.

The same three bright lines were seen. Their positions in the spectrum were verified by direct comparison with the induction-spark. In addition to these a spectrum could be traced from about D to about G of the solar spectrum. This spectrum is much stronger than the corresponding spectrum of 4373. This agrees with the greater brightness of the central star, or nucleus. The opinion that the faint continuous spectrum is formed alone by the light from the bright central point was confirmed by the following observation. When the cylindrical lens was removed, the three bright lines remained of considerable length, corresponding to the diameter of the telescopic image of the nebula; but the faint spectrum became as narrow as a line, showing that this spectrum is formed by light which comes from an object of which the image in the telescope is a point.

Lord Rosse remarks of this nebula, "A very remarkable object, perhaps analogous to H. 450"*.

The colour of this nebula is greenish blue.

[No. 4510. 2047 h. 51 H. IV. R.A. $19^h 36^m 3^s.0$. N.P.D.

* Philosophical Transactions, Part III. 1861, p. 733. For a figure of H. 450 see Philosophical Transactions, 1850, plate 38. fig. 15.

104° 28' 52".5. A planetary nebula. Bright; very small; round.] In Sagittarius.

This nebula is less bright than those which have been described. The two brighter of the lines were well defined, and were directly compared with the induction-spark. The third line was seen only by glimpses. I had a suspicion of an exceedingly faint spectrum.

The colour of this nebula is greenish blue.

Lord Rosse remarks, "Centre rather dark. The dark part is a little north preceding the middle"*.

(No. 4628. 2098 h. 1 H. IV. R.A. 20^h 56^m 31^s.2. N.P.D. 101° 55' 4".8. An exceedingly interesting object. Planetary; very bright; small; elliptic.] In Aquarius.

The three bright lines very sharp and distinct. They were compared for position with the induction-spark. Though this object is bright, an indication only of the faint spectrum was suspected. This nebula contains probably a very small quantity of matter condensed into the liquid or solid state.

The colour of the light of this nebula is greenish blue.

Lord Rosse has not detected any central star, nor any perforation as seen in some of the other planetary nebulae. He represents it with ansæ, which probably indicate a nebulous ring seen edgeways†.

[No. 4447. 2023 h. 57 M. R.A. 18^h 48^m 20^s. N.P.D. 57° 8' 57".2. An annular nebula; bright; pretty large; considerably elongated.] In Lyra‡.

The apparent brightness of this nebula, as seen in the tele-

* Philosophical Transactions, 1861, Part III. p. 732.

† Ibid. 1850, p. 507, and plate 38. fig. 14.

‡ Lord Rosse, in his description of this nebula, remarks, "The filaments proceeding from the edge become more conspicuous under increasing magnifying-power within certain limits, which is strikingly characteristic of a cluster; still I do not feel confident that it is resolvable."—Philosophical Transactions, 1844, p. 322, and plate 19. fig. 29.

In 1850 Lord Rosse further remarks, "I have not yet sketched it with the 6-feet instrument, because I have never seen it under favourable circumstances: the opportunities of observing it well on the meridian are comparatively rare, owing to twilight. It was observed seven times in 1848, and once in 1849. The only additional particulars I collect from the observations are that the central opening has considerably more nebulosity, and there is one pretty bright star in it, s. f. the centre, and a few other very minute stars. In the sky round the nebula and near it there are several very small stars which were not before seen; and therefore the stars in the dark opening may possibly be merely accidental. In the annulus, especially at the extremities of the minor axis, there are several minute stars, but there was still much nebulosity not seen as distinct stars."—Philosophical Transactions, 1850, p. 506.

"Nothing additional since 1844, except a star s. f. the middle."—Philosophical Transactions, 1861, p. 732.

scope, is probably due to its large extent; for the faintness of its spectrum indicates that it has a smaller intrinsic brightness than the nebulæ already examined. The brightest of the three lines was well seen. I suspected also the presence of the next in brightness. No indication whatever of a faint spectrum. The bright line looks remarkable, since it consists of two bright dots corresponding to sections of the ring; and between these there was not darkness, but an excessively faint line joining them. This observation makes it probable that the faint nebulous matter occupying the central portion is similar in constitution to that of the ring. The bright line was compared with the induction-spark*.

[No. 4964. 2241 h. 18 H. IV. R.A. 23^h 19^m 9^s.9. N.P.D. 48° 13' 57".5. Planetary; very bright; pretty small, round, blue.]

With a power of 600 this nebula appears distinctly annular. The colour of its light is greenish blue†. The spectrum formed by the light from this nebula corresponds with that of 37 H. IV. represented in fig. 5, Plate V.

In the spectrum of this nebula, however, in addition to the three bright lines, a fourth bright line, excessively faint, was seen. This line is about as much more refrangible than the line agreeing in position with F as this line is more refrangible than the brightest of the lines, which coincides with a line of nitrogen.

[No. 4532. 2060 h. 27 M. R.A. 19^h 53^m 29^s.3. N.P.D. 67° 39' 43". Very bright; very large; irregularly extended. Dumb-bell.] In Vulpecula.

The light of this nebula, after passing through the prisms, remained concentrated in a bright line corresponding to the brightest of the three lines represented in fig. 5, Plate V. This line appeared nebulous at the edges. No trace of the other lines was perceived, nor was a faint continuous spectrum detected.

The bright line was ascertained, by a simultaneous comparison with the spectrum of the induction-spark, to agree in position with the brightest of the lines of nitrogen.

* Already in 1850 Lord Rosse had discovered a connexion in general plan of structure between some of the nebulæ which present small planetary disks in ordinary telescopes, and the annular nebula in Lyra. His words are, "There were but two annular nebulæ known in the northern hemisphere when Sir John Herschel's Catalogue was published; now there are seven, as we have found that five of the planetary nebulæ are really annular. Of these objects, the annular nebula in Lyra is the one in which the form is the most easily recognized."—Philosophical Transactions, 1850, p. 506.

† For Lord Rosse's observations of this nebula, see Philosophical Transactions, 1844, p. 323; *ibid.* 1850, p. 507 and plate 38. fig. 13; *ibid.* 1861, p. 736 and plate 30. fig. 40.

Minute points of light have been observed in this nebula by Lord Rosse, Otto Struve, and others; the spectra of these bright points, especially if continuous like those of stars, are doubtless invisible from excessive faintness.

By suitable movements given to the telescope, different portions of the image of the nebula formed in the telescope were caused successively to fall upon the opening of the slit, which was about $\frac{1}{10}$ inch by $\frac{1}{300}$ inch. This method of observation showed that the light from different parts of the nebula is identical in refrangibility, and varies alone in degree of intensity*.

In addition to these objects the following were also observed:—

[No. 4294. 92 M. R.A. $17^h 12^m 56^s.9$. N.P.D. $46^\circ 43' 31''.2$.] In Hercules. Very bright globular cluster of stars. The bright central portion was brought upon the slit. A faint spectrum similar to that of a star. The light could be traced from between C and D to about G.

Too faint for the observation of lines of absorption.

[No. 4244. 50 H. IV. R.A. $16^h 43^m 6^s.4$. N.P.D. $42^\circ 8' 38''.8$. Very bright; large; round.] In Hercules. The spectrum similar to that of a faint star. No indication of bright lines.

[No. 116. 50 h. 31 M. R.A. $0^h 35^m 3^s.9$. N.P.D. $49^\circ 29' 45''.7$.] The brightest part of the great nebula in Andromeda was brought upon the slit.

The spectrum could be traced from about D to F. The light appeared to cease very abruptly in the orange; this may be due to the smaller luminosity of this part of the spectrum. No indication of the bright lines.

[No. 117. 51 h. 32 M. R.A. $0^h 35^m 5^s.3$. N.P.D. $49^\circ 54' 12''.7$. Very very bright; large; round; pretty suddenly much brighter in the middle.]

This small but very bright companion of the great nebula in Andromeda presents a spectrum apparently exactly similar to that of 31 M.

The spectrum appears to end abruptly in the orange, and,

* The author found, on November 23, 1864, that the light of the great nebula in Orion is resolved by the prism into three bright lines.—Proceedings of the Royal Society, vol. xiv. p. 39.

During 1865 he discovered that the nebulae which follow have similar spectra, consisting of one, two, or three bright lines, in addition to which, in the case of some of them, a continuous spectrum was visible.

2102....	27 H. IV.	4499....	38 H. VI.
4234....	5 Σ .	4827....	705 H. I.
4403....	17 M.	4627....	192 H. I.
4572....	16 H. IV.		

—Proceedings of the Royal Society, vol. xv. p. 18.

throughout its length, is not uniform but is evidently crossed either by lines of absorption or by bright lines.

[No. 428. 55 Androm. R.A. $1^h 44^m 55^s.9$. N.P.D. $49^\circ 57' 41''.5$. Fine nebulous star with strong atmosphere.] The spectrum apparently similar to that of an ordinary star*.

[No. 826. 2618 h. 26 IV. R.A. $4^h 7^m 50^s.8$. N.P.D. $103^\circ 5' 32''.2$. Very bright cluster.] In Eridanus. The spectrum could be traced from the orange to about the blue. No indication of the bright lines.

Several other nebulæ were observed, but of these the light was found to be too faint to admit of satisfactory examination with the spectrum-apparatus†.

It is obvious that the nebulæ 37 H. IV., 6 Σ ., 73 H. IV., 51 H. IV., 1 H. IV., 57 M., 18 H. IV., and 27 M. can no longer be regarded as aggregations of suns after the order to which our own sun and the fixed stars belong. We have in these objects to do no longer with a special modification only of our own type of suns, but find ourselves in the presence of objects possessing a distinct and peculiar plan of structure.

In place of an incandescent solid or liquid body transmitting light of all refrangibilities through an atmosphere which intercepts by absorption a certain number of them, such as our sun appears to be, we must probably regard these objects, or at least their photo-surfaces, as enormous masses of luminous gas or vapour. For it is alone from matter in the gaseous state that light consisting of certain definite refrangibilities only, as is the case with the light of these nebulæ, is known to be emitted.

It is indeed *possible* that suns endowed with these peculiar conditions of luminosity may exist, and that these bodies are clusters of such suns. There are, however, some considerations, especially in the case of the planetary nebulæ, which are scarcely in accordance with the opinion that they are clusters of stars.

Sir John Herschel remarks of one of this class, in reference to the absence of central condensation, "Such an appearance would not be presented by a globular space uniformly filled with stars or luminous matter, which structure would necessarily give rise to an apparent increase of brightness towards the centre, in proportion to the thickness traversed by the visual ray. We might therefore be inclined to conclude its real constitution to

* "Looked at eight times, but saw no nebulous atmosphere."—Lord Rosse, *Philosophical Transactions*, 1861, p. 712.

† The author has since observed 31 nebulæ and clusters, each of which gives a continuous spectrum.—*Proceedings of the Royal Society*, vol. xiv. p. 39; vol. xv. p. 18.

be either that of a hollow spherical shell or of a flat disk presented to us (by a highly improbable coincidence) in a plane precisely perpendicular to the visual ray”*. This absence of condensation admits of explanation, without recourse to the supposition of a shell or of a flat disk, if we consider them to be masses of glowing gas. For supposing, as we probably must do, that the whole mass of the gas is luminous, yet it would follow, by the law which results from the investigations of Kirchhoff, that the light emitted by the portions of gas beyond the surface visible to us, would be in great measure, if not wholly, absorbed by the portion of gas through which it would have to pass; and for this reason there would be presented to us a *luminous surface* only†.

Sir John Herschel further remarks‡, “Whatever idea we may form of the real nature of the planetary nebulæ, which all agree in the absence of central condensation, it is evident that the intrinsic splendour of their surfaces, if continuous, must be almost infinitely less than that of the sun. A circular portion of the sun’s disk subtending an angle of $1'$ would give a light equal to that of 780 full moons, while among all the objects in question there is not one which can be seen with the naked eye.” The small brilliancy of these nebulæ is in accordance with the conclusions suggested by the observations of this paper; for, reasoning by analogy from terrestrial physics, glowing or luminous gas would be very inferior in splendour to incandescent solid or liquid matter§.

* Outlines of Astronomy, 7th edit. p. 646.

† Sir William Herschel in 1811 pointed out the necessity of supposing the matter of the planetary nebulæ to have the power of intercepting light. He wrote:—“Admitting that these nebulæ are globular collections of nebulous matter, they could not appear equally bright if the nebulosity of which they are composed consisted only of a luminous substance perfectly penetrable to light. . . . Is it not rather to be supposed that a certain high degree of condensation has already brought on a sufficient consolidation to prevent the penetration of light, which by this means is reduced to a superficial planetary appearance?”

“Their planetary appearance shows that we only see a superficial lustre such as opaque bodies exhibit, and which could not happen if the nebulous matter had no other quality than that of shining, or had so little solidity as to be perfectly transparent.”—Philosophical Transactions, 1811, pp. 314, 315.

‡ Outlines of Astronomy, 7th edit. p. 646.

§ The author has made an attempt to determine approximately the *intrinsic* brightness of three of the gaseous nebulæ. It is probable that these bodies consist of continuous masses of material. In the telescope they present surfaces subtending a considerable angle. As long as a distant object is of sensible size in the telescope, its original brightness remains unaltered. By a suitable method of observation, the intensities of these

Such gaseous masses would be doubtless, from many causes, unequally dense in different portions; and if matter condensed into the liquid or solid state were also present, it would, from its superior splendour, be visible as a bright point or points within the disk of the nebula. These suggestions are in close accordance with the observations of Lord Rosse.

Another consideration which opposes the notion that these nebulæ are clusters of stars is found in the extreme simplicity of constitution which the three bright lines suggest, whether or not we regard these lines as indicating the presence of nitrogen, hydrogen, and a substance unknown.

It is perhaps of importance to state that, except nitrogen, no one of thirty of the chemical elements the spectra of which I have measured has a strong line very near the bright line of the nebulæ. If, however, this line were due to nitrogen, we ought to see other lines as well; for there are specially two strong double lines in the spectrum of nitrogen, one at least of which, if they existed in the light of the nebulæ, would be easily visible*. In my experiments on the spectrum of nitrogen, I found that the character of the brightest of the lines of nitrogen, that with which the line in the nebulæ coincides, differs from that of the two double lines next in brilliancy. This line is more nebulous at the edges, even when the slit is narrow and the other lines are thin and sharp. The same phenomenon was observed with some of the other elements†. We do not yet know the origin of this difference of character observable among lines of the same element. May it not indicate a physical difference in the atoms,

nebulae have been obtained in terms of the light of a sperm candle burning at the rate of 158 grains per hour.

The light of nebula 4628, 1 H. IV. =	$\frac{1}{1508}$	part of that of candle.
„ annular nebula, Lyra. =	$\frac{1}{6032}$	„ „
„ Dumb-bell nebula. . . =	$\frac{1}{19604}$	„ „

These values are too small by the unknown corrections for the possible power of extinction of space, and for the absorptive power of the earth's atmosphere.—Proceedings of the Royal Society, vol. xv. p. 18.

* Philosophical Transactions, 1864, p. 154 and plate 1.

For the purpose of ascertaining whether the absence of the other bright lines of nitrogen might be connected with the presence of hydrogen, I arranged an apparatus in which, while the spectrum of the induction-spark in a current of nitrogen was being observed, a current of hydrogen could be introduced, and the proportion of the two gases to each other easily regulated. With this apparatus the fading out of the bright lines of nitrogen, as the proportion of this gas to hydrogen was diminished, and again their increase in brilliancy when the current of nitrogen was made stronger, were carefully observed, but without detecting any marked variation in the relative brightness of the lines.

† Philosophical Transactions, 1864, pp. 143, 150.

in connexion with the vibrations of which the lines are probably produced? The speculation presents itself, whether the occurrence of this one line only in the nebulae may not indicate a form of matter more elementary than nitrogen, and which our analysis has not yet enabled us to detect*.

Observations on other nebulae, which I hope to make, may throw light upon these and other considerations connected with these wonderful objects.

LXXVI. *On the Level of the Sea during the Glacial Epoch.*
By Archdeacon PRATT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

SINCE I sent you a paper on the above subject, which was published in your Number for March, I have seen several letters which Mr. Croll's first letter has drawn forth; and I perceive that I have omitted to consider the effect of the drain upon the ocean which the formation of Mr. Croll's ice-sheet would cause. My calculation of the amount of rise in the level of the ocean in passing from the equator to northern latitudes may be all right. But the drain upon the ocean would reduce the level so as to make my starting-point (the equator) much lower, I conceive (probably 2000 feet), than the rise caused by the attraction of the ice-sheet. This therefore most materially affects the result.

This demand for water to form the ice-sheet would be supplied in part (but not sufficiently) from the southern hemisphere; for the attraction of the ice-sheet in the north would produce a depression in the ocean at the south pole and parts thereabout, and *by no means* an elevation.

I say "by no means," in reference to the way in which some of the writers and calculators in this problem have supposed that they see an analogy between the perturbation of the ocean by the moon and by the ice-sheet. But the cases are quite different. The moon is a free body, and as it draws the ocean so it draws the earth's centre; and the *difference* of effects must be taken to find the state of the ocean with reference to the earth's centre.

* On January 9, 1866, the author observed the spectrum of comet I. 1866. The comet appeared in his telescope as an oval nebulous mass surrounding a small and dim nucleus. The prism showed that the nucleus was self-luminous, that it consisted of matter in the state of ignited gas, and that *this matter is similar in constitution to the gaseous material of some of the nebulae*. The coma shines by reflected solar light.—Proceedings of the Royal Society, vol. xv. p. 5.

Not so with the ice-sheet. The ice-sheet is rigidly attached to the earth, which the moon is not. It may produce a strain in the solid materials of the earth by its attraction, but cannot draw the earth's centre to it. Hence the ice-sheet will have its whole effect on the ocean, which must not be diminished by theoretically applying a force equal to the attraction on the centre in the opposite direction.

It may be said, But the moon does *not* actually draw the earth's centre to it. If this be true, it is because the earth-and-moon's revolving round their common centre of gravity produces a centrifugal force in each, so as to keep each in its place; and this centrifugal force affects not only the centre, but the ocean too.

It is not, however, strictly true that the moon does not draw the earth's centre to it. This would be the case if the orbit were a circle; but it is more like an ellipse, the moon sometimes receding, sometimes approaching the earth's centre. This of itself might perhaps be a sufficient answer to the writer who says that the moon's action on the earth and ocean would not be affected if it were rigidly connected with the earth, and therefore why may not the ice-sheet be considered to be like the moon in its effect in raising the water opposite to it as well as under and near it? But the proper answer is, that the ice-sheet's being rigidly connected with the earth's centre produces a force which keeps them at the same distance from each other, *and* that force has no effect whatever (as the centrifugal force between the moon and earth has) on the ocean. In fact, to return to what I said at first, the moon is a free body with reference to the earth, but the ice-sheet is a fixed body.

J. H. PRATT.

Calcutta, April 20, 1866.

LXXVII. *On the Observations and Calculations required to find the Tidal Retardation of the Earth's Rotation.* By Professor W. THOMSON, F.R.S.*

THE first publication of any definite estimate of the possible amount of the diminution of rotatory velocity experienced by the earth through tidal friction is due, I believe, to Mr. William Ferrel, and is to be found in the Number for December 8, 1853, of the *Astronomical Journal* of Cambridge, United States. It is founded on calculating the moment round the earth's centre of the attraction of the moon, on a regular spheroidal shell of water symmetrical about its longest axis, this being (through the influence of fluid friction) kept in a position inclined backwards

* From the Rede Lecture, Cambridge, May 23, 1866, "On the Dissipation of Energy." Communicated by the Author.

Phil. Mag. S. 4. No. 212. Suppl. Vol. 31.

at an acute angle to the line from the earth's centre to the moon. One of the simplest ways of seeing the result is this:—First, by the known conclusions as to the attractions of ellipsoids, or still more easily by the consideration of the proper “spherical harmonic”* (or Laplace's coefficient) of the second degree, we see that an equipotential surface lying close to the bounding surface of a nearly spherical homogeneous solid ellipsoid is approximately an ellipsoid with axes differing from one another by three-fifths of the amounts of the differences of the corresponding axes of the ellipsoidal boundary. From this it follows† that a homogeneous prolate spheroid of revolution attracts points outside it approximately as if its mass were collected in a uniform bar having its ends in the foci of the equipotential spheroid. If, for example, a globe of water of 21,000,000 feet radius (this being nearly enough the earth's radius) be altered into a prolate spheroid with longest radii exceeding the shortest radii by two feet, the equipotential spheroid will have longest and shortest radii differing by $\frac{6}{5}$ of a foot. The foci of this latter will be at 7100 feet on each side of the centre; and therefore the resultant of gravitation between the supposed spheroid of water and external bodies will be the same as if its whole mass were collected in a uniform bar of 14,200 feet length. But by a well-known proposition‡, a uniform line FF' (a diagram is unnecessary) attracts a point M in the line MK bisecting the angle FMF' . Let CQ be a perpendicular from C , the middle point of $F'F$, to this bisecting line MK . If CM be $60 \times 21 \times 10^6$ (the moon's distance), and if the angle FCM be 45° , we find, by elementary geometry, $CQ = .02$ of a foot (about $\frac{1}{4}$ inch). The mass of a globe of water equal in bulk to the earth is 1.1×10^{21} tons§. And, the moon's mass being about $\frac{1}{75}$ of the earth's, the attraction of the moon on a ton at the earth's distance is $\frac{1}{75} \times \frac{1}{60^2}$, or $\frac{1}{270,000}$ of a ton force, if, for brevity, we call a ton force the ordinary terrestrial weight of a ton—that is to say, the amount of the earth's attraction on a ton at its surface. Hence the whole force of the moon on the earth is $\frac{1.1 \times 10^{21}}{270,000}$, or 4.1×10^{15} tons force. If, then, the tidal disturbance were exactly what we have supposed, or if it were (however irre-

* Thomson and Tait's 'Natural Philosophy,' § 536 (4).

† Ibid. § 501 and § 480 (e).

‡ Ibid. § 480 (b) and (a).

§ In stating large masses, if English measures are used at all, the ton is convenient, because it is 1000 kilogrammes nearly enough for many practical purposes and rough estimates. It is 1016.047 kilogrammes; so that a ton diminished by about 1.6 per cent. would be just 1000 kilogrammes.

gular) such as to have the same resultant effect, the retarding influence of the moon's attraction would be that of 4.1×10^{15} tons force acting in the plane of the equator and in a line passing the centre at $\frac{1}{50}$ of a foot distance. Or it would be the same as a simple frictional resistance (as of a friction-brake) consisting of 4.1×10^{15} tons force acting tangentially against the motion of a pivot or axle of about $\frac{1}{2}$ inch diameter. To estimate the retardation produced by this, we shall suppose the square of the earth's radius of gyration, instead of being $\frac{2}{5}$, as it would be if the mass were homogeneous, to be $\frac{1}{3}$ of the square of the radius of figure, as it is made to be, by Laplace's probable law of the increasing density inwards, and by the amount of precession calculated on the supposition that the earth is quite rigid. Hence (if we take $g=32.2$ feet per second generated per second, and the earth's mass as 6.1×10^{21} tons) the loss of angular velocity per second, on the other suppositions we have made, will be

$$\frac{32.2 \times 4.1 \times 10^{15} \times .02}{6.1 \times 10^{21} \times \frac{1}{3}(21 \times 10^6)^2}, \text{ or } 2.94 \times 10^{-21}.$$

The loss of angular velocity in a century would be $31\frac{1}{2} \times 10^8$ times this, or $.93 \times 10^{-11}$, which is as much as $\frac{1.28}{10^7}$ of

$\frac{2\pi}{86400}$, the present angular velocity. Thus in a century the earth would be rotating so much slower that, regarded as a time-keeper, it would lose about one second and a quarter in ten million, or four seconds in a year. And the accumulation of effect of uniform retardation at that rate would throw the earth as a time-keeper behind a perfect chronometer (set to agree with it in rate and absolute indication at any time) by 200 seconds at the end of a century, 800 seconds at the end of two centuries, and so on. In the present very imperfect state of clock-making (which scarcely produces an astronomical clock two or three times more accurate than a marine chronometer or good pocket-watch), the only chronometer by which we can check the earth is one which goes much worse—the moon. The marvellous skill and vast labour devoted to the lunar theory by the great physical astronomers Adams and Delaunay, seem to have settled that the earth has really lost in a century about ten seconds of time on the moon corrected for perturbations. M. Delaunay has suggested that the true cause may be tidal friction, which he has proved to be probably sufficient by some such estimate as the preceding*. But the many disturbing influences to which

* It seems hopeless, without waiting for some centuries, to arrive at

the earth is exposed render it a very untrustworthy time-keeper. For instance, let us suppose ice to melt from the polar regions (20° round each pole, we may say) to the extent of something more than a foot thick, enough to give 1.1 foot of water over those areas, or .066 of a foot of water if spread over the whole globe, which would in reality raise the sea-level by only some such almost undiscoverable difference as $\frac{3}{4}$ of an inch, or an inch. This, or the reverse, which we may believe might happen any year, and could certainly not be detected without far more accurate observations and calculations for the mean sea-level than any hitherto made, would slacken or quicken the earth's rate as a time-keeper by one-tenth of a second per year*.

Again, an excellent suggestion, supported by calculations which show it to be not improbable, has been made to the French Academy by M. Dufour, that the retardation of the earth's rotation indicated by M. Delaunay, or some considerable part of it, may be due to an increase of its moment of inertia by the incorporation of meteors falling on its surface. If we suppose the previous average moment of momentum of the meteors round the earth's axis to be zero, their influence will be calculated just as I have calculated that of the supposed melting of ice. Thus meteors falling on the earth in fine powder (as is in all probability the lot of the greater number that enter the earth's atmosphere and do not escape into external space again) enough to form a layer about $\frac{1}{20}$ of a foot thick in 100 years, if of twice the density of water, would produce the supposed retardation of

any approach to an exact determination of the amount of the actual retardation of the earth's rotation by tidal friction, except by extensive and accurate observation of the amounts and times of the tides on the shores of continents and islands in all seas, and much assistance from *true* dynamical theory to estimate these elements all over the sea. But supposing them known for every part of the sea, the retardation of the earth's rotation is easily calculated by quadratures.

* The calculation is simply this. Let E be the earth's whole mass, a its radius, k its radius of gyration before, and k' after the supposed melting of the ice, and W the mass of ice melted. Then, since $\frac{2}{3}a^2$ is the square of the radius of gyration of the thin shell of water supposed spread uniformly over the whole surface, and that of either ice-cap is, very approximately $\frac{1}{2}a^2 (\sin 20^\circ)^2$, we have

$$Ek'^2 = Ek^2 + Wa^2 \left[\frac{2}{3} - \frac{1}{2} (\sin 20^\circ)^2 \right].$$

And, by the principle of the conservation of moments of momentum, the rotatory velocity of the earth will vary inversely as the square of its radius of gyration. To put this into numbers, we take, as above, $k^2 = \frac{1}{3}a^2$ and $a = 21 \times 10^6$. And as the mean density of the earth is about $5\frac{1}{2}$ times that of water, and the bulk of a globe is the area of its surface into $\frac{1}{3}$ of its radius,

$$E : W :: \frac{5.5a}{3} : .066.$$

10^s on the time shown by the earth's rotation. But this would also accelerate the moon's mean motion by half the same proportional amount; and therefore a layer of meteor-dust accumulating at the rate of $\frac{1}{30}$ of a foot per century, or 1 foot in 3000 years, would suffice to explain Messrs. Adams and Delaunay's result. I see no other way of directly testing the probable truth of M. Dufour's very interesting hypothesis than to chemically analyze quantities of natural dust taken from any suitable localities (such dust, for instance, as has accumulated in two or three thousand years to depths of many feet over Egyptian, Greek, and Roman monuments). Should a considerable amount of iron with a large proportion of nickel be found or not found, strong evidence for or against the meteoric origin of a sensible part of the dust would be afforded.

Another source of error in the earth as a time-keeper, which has often been discussed, is its shrinking by cooling. But I find by the estimates I have given elsewhere* of the present state of deep underground temperatures, and by taking $\frac{1}{100000}$ as the vertical contraction per degree Centigrade of cooling in the earth's crust, that the gain of time by the earth, regarded as a clock, would not in a century amount to more than $\frac{1}{30}$ of a second, or $\frac{1}{6000}$ of the amount estimated above as conceivably due to tidal friction.

LXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 477.]

March 15, 1866.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

“On a possible Geological Cause of Changes in the Position of the Axis of the Earth's Crust.” By John Evans, F.R.S., Sec. G.S.

At a time when the causes which have led to climatal changes in various parts of the globe are the subject of so much discussion, but little apology is needed for calling the attention of this Society to what possibly may have been one of these causes, though it has apparently hitherto escaped observation.

That great changes of climate have taken place, at all events in the northern hemisphere of the globe, is one of the best-established facts of geology, and that corresponding changes have not been noticed to the same extent in the southern hemisphere may possibly be considered due rather to a more limited amount of geological observation than to an absence of the phenomena indicative of such alterations in climatal conditions having occurred.

The evidence of the extreme refrigeration of this portion of the

* “Secular Cooling of the Earth.” Transactions of the Royal Society of Edinburgh, 1862; and Philosophical Magazine, January 1863.

earth at the Glacial Period is constantly receiving fresh corroboration ; and various theories have been proposed which account for this accession of cold in a more or less satisfactory manner.

Variations in the distribution of land and water, changes in the direction of the Gulf-stream, the greater or less eccentricity of the earth's orbit, the passage of the Solar System through a cold region in space, fluctuations in the amount of heat radiated by the sun, alternations of heat and cold in the northern and southern hemispheres, as consequent upon the precession of the equinoxes, and even changes in the position of the centre of gravity of the earth, and consequent displacements of the polar axis, have all been adduced as causes calculated to produce the effects observed ; and the reasoning founded on each of these data is no doubt familiar to all.

The possibility of any material change in the axis of rotation of the earth has been so distinctly denied by Laplace* and all succeeding astronomers, that any theory involving such a change, however tempting as affording a solution of certain difficulties, has been rejected by nearly all geologists as untenable.

Sir Henry James†, however, writing to the 'Athenæum' newspaper in 1860, stated that he had long since arrived at the conclusion that there was no possible explanation of some of the geological phenomena testifying to the climate at certain spots having greatly varied at different periods, without the supposition of constant changes in the position of the axis of the earth's rotation. He then, assuming as an admitted fact that the earth is at present a fluid mass with a hardened crust, showed that slaty cleavage, dislocations, and undulations in the various strata are results which might be expected from the crust of the earth having to assume a new external form, if caused to revolve on a new axis, and advanced the theory that the elevation of mountain-chains of larger extent than at present known produced these changes in the position of the poles.

The subject was discussed in further letters from Sir Henry James, the Astronomer Royal, Professors Beete Jukes and Hennessy, and others ; but throughout the discussion the principal question at issue seems to have been whether any elevation of a mountain-mass could sensibly affect the position of the axis of rotation of the globe as a whole ; and the general verdict was in the negative.

- At an earlier period (1848) the late Sir John Lubbock, in a short but conclusive paper in the 'Quarterly Journal of the Geological Society'‡ pointed out what would have been the effect had the axis of rotation of the earth not originally corresponded with the axis of figure, and also mentioned some considerations which appear to have been absent from Laplace's calculations.

Sir John Lubbock, however, in common with other astronomers, appears to have regarded the earth as consisting of a solid nucleus with a body of water distributed over a portion of its surface ; and there can be but little doubt that, on this assumption of the solidity of the earth, the usually received doctrines as to the general persistence of the direction of the poles are almost unassailable.

* *Mécanique Céleste*, vol. v. p. 14.

† *Athenæum*, Aug. 25, 1860, &c.

‡ Vol. v. p. 5.

Directly, however, that we argue from the contrary assumption, that the solid portion of the globe consists of a comparatively thin but to some extent rigid crust, with a fluid nucleus of incandescent mineral matter within, and that this crust, from various causes, is liable to changes disturbing its equilibrium, it becomes apparent that such disturbances may lead, if not to a change in the position of the general axis of the globe, yet at all events to a change in the relative positions of the solid crust and the fluid nucleus, and in consequence to a change in the axis of rotation, so far as the former is concerned.

The existence in the centre of the globe of a mass of matter fluid by heat, though accepted as a fact by many (if not most) geologists, has no doubt been called in question by some, and among them a few of great eminence. The gradual increase of temperature, however, which is found to take place as we descend beneath the surface of the earth, and which has been observed in mines and deep borings all over the world, the existence of hot springs, some of the temperature of boiling water, and the traces of volcanic action, either extinct or still in operation, which occur in all parts of the globe, afford strong arguments in favour of the hypothesis of central heat.

And though we are at present unacquainted with the exact law of the increment of heat at different depths, and though, no doubt, under enormous pressure the temperature of the fusing-point of all substances may be considerably raised, yet the fact of the heat increasing with the depth from the surface seems so well established that it is highly probable that at a certain depth such a degree of heat must be attained as would reduce all mineral matter with which we are acquainted into a state of fusion. When once this point was attained, it seems probable that there would be no very great variation in the temperature of the internal mass; but whether the whole is in one uniform state of fluidity, or whether there is a mass of solid matter in the centre of the fluid nucleus, are questions which do not affect the hypothesis about to be considered.

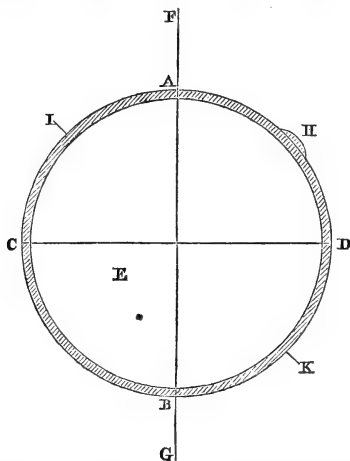
Those who are inclined to regard the earth as a solid or nearly solid mass throughout, consider that many volcanic phenomena may be accounted for on the chemical theory, which has received the support, among others, of Sir Charles Lyell. But apart from the consideration that such chemical action must of necessity be limited in its duration, the existence of local seas of fluid matter, resulting from the heat generated by intense chemical action, would hardly account for the increase of heat at great depths in places remote from volcanic centres; and the rapid transmission of shocks of earthquakes and the enormous amount of upheaval and subsidence as evidenced by the thickness of the sedimentary strata, seem inconsistent either with the general solidity of the globe or any very great thickness of its crust.

The supposition that the gradual oscillations of the surface of the earth, of which we have evidence all over the world as having taken place ever since the formation of the earliest known strata up to the present time, are due to the alternate inflation by gas and the subsequent depletion of certain vast bladdery cavities in the crust of the earth, can hardly be generally accepted.

Those who wish to see the arguments for and against the theory of there being a fluid nucleus within the earth's crust, will find them well and fairly stated in Naumann's '*Lehrbuch der Geognosie*'*. My object is, not to discuss that question, but to point out what, assuming the theory to be true, would be some of the effects resulting from such a condition of things, more especially as affecting climatal changes. The agreement or disagreement between these hypothetical results and observed facts may ultimately assist in testing the truth of the assumption.

The simplest form in which we can conceive of the relations to each other of a solid crust and a fluid nucleus in rotation together is that of a sphere.

Let *ACBD* be a hollow sphere composed of solid materials and of perfectly uniform thickness and density, and let it be filled with the fluid matter *E*, over which the solid shell can freely move, and let the whole be in uniform rotation about an axis *FG*, the line *CD* representing the equator. It is evident that in such a case, the



hollow sphere being in perfect equilibrium, its axis and that of its fluid contents would perpetually coincide. If, however, the equilibrium of the shell or crust be destroyed, as, for instance, by the addition of a mass of extraneous matter at *H*, midway between the pole and the equator, not only would the position of the axis of rotation be slightly affected by the alteration in the position of the centre of gravity of the now irregular sphere, but the centrifugal force of the excess of matter at *H* would gradually draw over the shell towards *D* until, by sliding over the nucleus, it attained its greatest possible distance from the centre of revolution by arriving at the equator. The resultant effect would be that though the whole sphere continued to revolve around an axis as nearly as possible in the line *FG*, yet the position of the pole of the hollow shell would have been changed by 45° , as by the passage of *H* to

* 2nd edit., 1858, vol. i. p. 36.

the equator the points I and K would have been brought to the poles by spirals constantly decreasing in diameter, while A and B, by spirals constantly increasing, would have at last come to describe circles midway between the poles and the equator.

The axis of rotation of the hollow sphere and that of its fluid contents would now again coincide, and would continue to do so perpetually unless some fresh disturbance in the equilibrium of the shell took place.

If instead of the addition of fresh matter at H we had supposed an excavation or removal of some portion of the shell, a movement in the axis of rotation of the shell would also have ensued, since from the diminished centrifugal force of that portion of the hollow sphere where the excavation had taken place, it would no longer equipoise the corresponding portion on the opposite side at I, and the excavated spot would eventually find its way to the pole.

In order more clearly to exhibit these effects, I have prepared a model in accordance with a suggestion of Mr. Francis Galton, F.R.S., in which a wheel representing a section of a hollow sphere has its axis, upon which it can freely turn, fixed in a frame, which is itself made to revolve in such a manner that the axis of its rotation passes through one of the diameters of the wheel, and coincides with what would be the axis of the sphere of which the wheel is a section.

In the periphery of the wheel are a number of adjustable screws with heavy heads, so that, by screwing any of them in or out, the addition of matter or its abstraction at any part of the sphere may be represented.

If by adjusting these screws the wheel could be brought into perfect equilibrium, its position upon its own axis would remain unchanged in whatever position it was originally placed, notwithstanding any amount of rotation being given to the frame in which it is hung; but practically it is found that with a certain given position of the screws a certain part of the wheel coincides with the axis of the frame, or becomes the pole around which the sphere revolves. The rim of the wheel is graduated so as to show the position of the poles in all cases, and generally speaking the wheel always settles down after rotation with the pole within three or four degrees of the same spot, if no alteration has been made in the adjustment of the screws, though of course what was the uppermost pole may become the lower one; and in some cases the wheel may be *in æquilibrio* with a projecting screw either above or below the equator, in which case there may be four readings on the circle at the index-point, according as the one pole or the other is uppermost, and the projecting screw is above or below the equator.

With the screws on the wheel evenly balanced, a slight alteration in the adjustment of any of them immediately tells upon the position of what, for convenience sake, may be called the poles, except, indeed, in such cases as screwing outwards those already at the equator, or making similar alterations in the adjustment of two screws at equal distances on either side of one of the poles. If a screw be turned outwards so as notably to project at any spot, no matter how near to the pole, it will be found, after the machine has been a short time

in revolution, in the region of the equator. Or again, if one or, better still, two opposite screws at the equator be turned inwards, they will be found after a short period of revolution at the poles.

Now let us assume for a moment that, though the crust was partially covered by water, the earth, instead of being a spheroid, was a perfect sphere, consisting of a hardened crust of moderate thickness supported on a fluid nucleus over which the crust could travel freely in any direction, but both impressed with the same original rotatory motion, so that without some disturbing cause they would continue to revolve for ever upon the same axis, and as if they were one homogeneous body. Let us assume, moreover, that this crust, though in perfect equilibrium on its centre of rotation, was not evenly spherical externally, but had certain projecting portions, such as would be represented in Nature by continents and islands rising above the level of the sea.

It is evident that so long as those continents and islands remained unaltered in their condition and extent, the relative position of the crust to the enclosed fluid nucleus would remain unaltered also. But supposing those projecting masses were either further upheaved from some internal cause, or worn down and ground away by the sea or by subaërial agency and deposited elsewhere, it seems impossible but that the same effects must ensue as we see resulting upon the model from the elevation and depression of certain screws, and that the axis of rotation of the crust of the sphere would be changed in consequence of its having assumed a fresh position upon its fluid nucleus, though the axis of the whole sphere might have retained its original direction, or have altered from it only in the slightest degree.

An irregular accumulation of ice at one or both of the poles, such as is supposed by M. Adhémar, would act in the same manner as an elevation of the land; and even assuming that the whole land had disappeared from above the surface of the sea, yet if by marine currents the shallower parts of the universal ocean were deepened and the deeper parts filled up, there would, owing to the different specific gravity of the transported soil and the displaced water, be a disturbance in the equilibrium of the crust, and a consequent change in the position of its axis of rotation.

Now if all this be true of a sphere, it will also, subject to certain modifications, be true of a spheroid so slightly oblate as our globe.

The main difference in the two cases is, that in a sphere the crust may assume any position upon the nucleus without any alteration in its structure, while in the case of the movement of a spheroidal crust over a similar spheroidal nucleus, every portion of its internal structure must be more or less disturbed, as the curvature at each point will be slightly altered.

The extent of the resistance to an alteration of position arising from this cause will depend upon the oblateness of the spheroid and the thickness and rigidity of the crust; while the thicker the latter is, the less also will be the proportionate effect of such elevations, subsidences, and denudations as those with which we are acquainted. The question of friction upon the nucleus is also one that would have to be considered, as the internal matter though fluid might be viscous.

It will of course be borne in mind that the elevations and depressions of the surface of the globe are not, on the theory now under consideration, regarded according to the proportion they bear to the earth's radius, but according to their relation to the thickness of the earth's crust; and that, even assuming Mr. Hopkins's extreme estimate to be true, yet elevations or depressions, such as we know to have taken place, of 8000 or 10,000 feet, bear an appreciable ratio to the 800 or 1000 miles which he assigns as the thickness of the earth's crust.

It is, however, to be remarked that the extremely ingenious speculations of Mr. Hopkins are based on the phenomena of precession and nutation, and that if once the possibility of a change in the position of the axis of rotation of the earth's crust be admitted, it is not improbable that the value of some of the data upon which the calculations of these movements are founded may be affected.

The supposition of the thickness of the crust being so great seems also not only entirely at variance with observed facts as to the increase of heat on descending beneath the surface of the earth, but to have been felt by Mr. Hopkins himself to offer such obstacles to any communication between the surface of the globe and its interior, that he has had recourse to an hypothesis of large spaces in the crust at no great depth from the surface, and filled with easily-fusible materials, in order to account for volcanic and other phenomena.

But though it may be possible to account for volcanoes upon such an assumption, yet, as already observed, the phenomena of elevation and depression, such as we find to have taken place, and more especially the existence of vast geological faults, cannot without enormous difficulty be reconciled with such a theory.

Taking the increment of heat as 1° Fahrenheit for every 55 or 60 feet* in descent, a temperature of 2400° Fahr. would be reached at about 25 miles, sufficient to keep in fusion such rocks as basalt, greenstone, and porphyry; and such a thickness appears much more consistent with the fluctuations in level, and the internal contortions and fractures of the crust which are everywhere to be observed. Sir William Armstrong, on the assumption of the temperature of subterranean fusion being 3000° Fahr., considers that the thickness of the film which separates us from the fiery ocean beneath would be about 34 miles.

Even assuming a thickness of 50 miles, so as to make still greater allowance for the increased difficulty of fusion under heavy pressure, the thickness of the crust would only form one-eightieth part of the radius of the earth; or if we represent the earth by a globe 13 feet in diameter, the crust would be one inch in thickness, while the difference between the polar and equatorial diameters would be half an inch.

In such a case, the elevation or wearing away of continents such as are at present in existence, rising, as some of them do, nearly a quarter of a mile on an average above the mean sea-level, would cause a great disturbance in the equilibrium of the crust, sufficient

* Page, 'Advanced Text-book of Geology,' p. 30.

to overcome considerable resistance in its attempts to regain a state of equilibrium by a movement over its fluid nucleus.

Whether the thickness of the earth's crust was not in early geological times less than at present, so as to render it more susceptible of alterations in position—whether the spheroid of the fluid mineral nucleus corresponds in form with the spheroid of water which gives the general contour of the globe—whether or no there are elevations and depressions upon the nucleus corresponding to some extent with the configuration of the outer crust, and whether the motion of the crust upon it, besides effecting climatal changes, might not also lead to some elevations and depressions of the land, and produce some of the other phenomena mentioned by Sir Henry James, are questions which I will leave for others to discuss.

My object is simply to call attention to what appears to me the fact, that if, as there seems reason to suppose, our globe consists of a solid crust of no great thickness resting on a fluid nucleus, either with or without a solid central core, and if this crust, as there is abundant evidence to prove, is liable to great disturbances in its equilibrium, then it of necessity follows that changes take place in the position of the crust with regard to the nucleus, and an alteration in the position of the axis of rotation, so far as the surface of the earth is concerned, ensues.

Without in the slightest degree undervaluing other causes which may lead to climatal changes, I think that possibly we may have here a *vera causa* such as would account for extreme variations from a Tropical to an Arctic temperature at the same spot, in a simpler and more satisfactory manner than any other hypothesis.

The former existence of cold in what are now warm latitudes might, and probably did in part, arise from other causes than a change in the axis of rotation, but no other hypothesis can well account for the existence of traces of an almost tropical vegetation within the Arctic circle.

Of the former existence of such a vegetation, the evidence, though strong, is not conclusive. But if the fossil plants of Melville Island, in lat. 75° N.*, which appear to agree generically with those from the English coal-measures, really grew upon the spot where they were now discovered, they seem to afford conclusive evidence of a change in the position of the pole since the period at which they grew, as such vegetation must be considered impossible in so high a latitude.

The corals and Orthoceratites from Griffiths Island and Cornwallis Island, and the liassic Ammonites from Point Wilkie, Prince Patrick's Island, tell the same story of the former existence of something like a subtropical climate at places at present well within the Arctic circle.

To use the words of the Rev. Samuel Haughton†, in describing the fossils collected by Sir F. L. McClintock, "The discovery of such fossils *in situ*, in 76° N. latitude, is calculated to throw considerable doubt upon the theories of climate, which would account for all past changes of temperature by changes in the relative position

* Lyell, 'Principles of Geology,' 1853, p. 88.

† Journal of the Royal Dublin Society, vol. i. p. 244.

of land and water on the earth's surface;" and I think that all geologists will agree with this remark, and feel that if the possibility of a change in the position of the axis of rotation of the crust of the earth were once admitted, it would smooth over many difficulties they now encounter.

That some such change is indeed taking place at the present moment may not unreasonably be inferred from the observations of the Astronomer Royal, who, in his Report to the Board of Visitors for 1861, makes use of the following language, though "only for the sake of embodying his description of the observed facts," as he refers the discrepancies noticed to "some peculiarity of the instrument. . . . The Transit Circle and Collimators still present those appearances of agreement between themselves and of change with respect to the stars which seem explicable only on one of two suppositions—that the ground itself shifts with respect to the general Earth, or that the Axis of Rotation changes its position."

GEOLOGICAL SOCIETY.

[Continued from p. 482.]

April 25, 1866.—Warington W. Smyth, Esq., President, in the Chair.

The following communications were read:—

1. "Additional documents relating to the Volcanic Eruptions at the Kaimeni Islands." By Commander Brine, of H.M.S. 'Racer.'

In these documents it was stated that the active volcano now forming part of Neo Kaimeni Island continues to increase in size by the addition of volcanic matter ejected from the crater, and that the rate of increase of the new island situated to the south-west, near St. George's Bay, is considerably less than at first. The new island contains the crater of a second volcano, 30 feet in height, with a circular base of 300 yards; and, judging from the soundings obtained at Paleo Kaimeni and St. George's Bay, it is probable that the island will eventually fill up the bay.

2. "Report to the Eparch of Santorino on the Eruptions at the Kaimeni Islands." By M. Fouqué.

Since the eruptions at Santorino earthquakes have become much less violent in the surrounding country, and the fears of the inhabitants have been unnecessarily great. A new fissure has been opened between George Island and Aphroessa; and lava and torrents of steam have issued from this vent, as well as much gas. The non-existence of a crater was considered by M. Fouqué to be due to the small quantity of ejected matter and the feebleness of the eruption. M. Ste.-Claire Deville has shown that there exists a certain relation between the degree of intensity of a volcano in action and the nature of the volatile elements ejected; and M. Fouqué has been enabled to establish the truth of this law. Thus, in an eruption of maximum intensity, the predominant volatile product is chloride of sodium, accompanied by the salts of soda and potash; an eruption of the second order gives hydrochloric acid and chloride of iron; in the third degree, sulphuric acid and salts of ammonia; and in the fourth

or most feeble phase; steam only, with carbonic acid and combustible gases. The eruption at Neo Kaimeni has never exceeded the third degree of intensity; and when it excited the greatest alarm, it gave off only sulphuric acid, steam, and combustible gases.

3. "Remarks upon the Interval of Time which has passed between the formation of the Upper and Lower Valley-gravels of part of England and France; with notes on the character of the Holes bored in rocks by Mollusca." By A. Tylor, Esq., F.G.S.

The difficulties attending investigations into the relative ages of gravel-deposits having been stated, and a *résumé* given of the steps by which the opinions now current on the subject had been arrived at, Mr. Tylor proceeded to combat the view that the Upper and Lower Valley-gravels are separated from each other by a long interval of time. The conclusion that man had existed on the earth from so distant a date as is required by Mr. Prestwich's interpretation of the phenomena exhibited in the valleys of the Somme and other rivers was also considered untenable, and to prove that the theory requiring it is erroneous.

Accepting Mr. Godwin-Austen's theory of the Pleistocene age of the English Channel, the author inferred from it that the excavation of the transverse valleys of the south-east of England was similar to that of the valleys of Devonshire, which he considers to have been excavated in remote geological periods, and to have been filled with gravel prior to the period of the valley-gravels, at which time the valleys were re-excavated. He then brought forward evidence to show that, in the case of the small valley in which Kent's Hole (180 feet above the sea-level) is situated, the gravel has been swept away from the valley during an epoch immediately preceding the historic period, and without any appearance of great denudation of the older rocks, leaving what may be called High- and Low-level Valley-gravels on its slopes as remanié deposits; and in support of this view he mentioned the presence of human implements in these gravels, the existence of *Pholas*-perforations on the face of the rock in which are the two openings of Kent's Hole (showing that little weathering had taken place since), as well as the occurrence of a bed of red clay, or loess, 80 feet thick, and 220 feet at its base above the sea-level.

The age of the Kent's Hole Valley was identified with that of the Valley of the Somme, on account of the similar position of the gravels and of the raised beaches at the coast-line, as well as the similarity of levels and of the organic contents of the detritus in the two valleys.

In conclusion Mr. Tylor gave a note on the character of holes bored in rocks by Mollusca, with especial reference to the bored rocks at Kent's Hole and Marychurch, about 200 feet above the present sea-level, coming to the conclusion that they have probably been formed by *Pholas dactylus*.

May 9.—Warrington W. Smyth, Esq., M.A., F.R.S., Pres., in the Chair.

The following communications were read:—

1. "On a new species of *Acanthodes* from the Coal-shales of

Longton." By Sir Philip de M. Grey Egerton, Bart., M.P., F.R.S., F.G.S.

Owing to the kindness of Mr. Ward, of Longton, the author had been enabled to examine a considerable collection of specimens of the Acanthodean fishes of the North Staffordshire Coal-field. The specimens were all imperfect, the anterior parts of the fish being rarely preserved, and even when present being in a very mutilated condition; but Sir Philip Egerton had been able to determine the distinctness of at least one species, which he now described as *Acanthodes Wardi*. This species was far less bulky and more elongated than *A. Bronni* from the Saarbrück Coal-field; but it was not so slender as *A. gracilis* from the Permian beds of Klein Neudorf.

2. "A sketch of the Gravels and Drift of the Fenland." By Harry Seeley, Esq., F.G.S.

By the Fenland was understood the flat country west of the Chalk Hills of Norfolk, from Hunstanton to Cambridge, thence to Bedford, and northwards to Peterborough. Three kinds of Drift were described as occurring in this region—namely, Boulder-clay covering the high land, a coarse gravel which caps the hills, and the fine gravel of the plains. Mr. Seeley gave first a sketch of their distribution over the area under consideration, and then described some of their most important exposures, especially the sections at March, Barnwell, and Hunstanton. He also gave lists of the marine shells found at March, occurring between Boulder-clays, and those found at Hunstanton, which are of much later date; also of the bones and land and freshwater shells found at Barnwell, including one bone described as having been cut by man previous to deposition in the gravel.

Comparing the drift of the Fenland with that of the Eastern Counties, Mr. Seeley inferred that the brown clay of the latter district corresponds with the brown boulder-clay, which is the oldest drift-deposit in the former, and that the hill-gravel, the blue boulder-clay, and perhaps the shell-bed of March, correspond to the Contorted Drift.

3. "Additional Observations on the Geology of the Lake-country." By Prof. R. Harkness, F.R.S., F.G.S., and H. Nicholson, Esq. With a Note on the Trilobites; by J. W. Salter, Esq., F.G.S.

The authors having first communicated the following additions to the fauna of the Skiddaw slates—namely, from the lower strata, *Phacops Nicholsoni*, n. sp., *Æglina binodosa*, and *Lingula brevis*; and from the upper beds, *Diplograpsus teretiusculus* and *Agnostis morea*—they stated that fossiliferous rocks had been discovered by them among the "ash-beds" of the Lake-country on the same horizon as those associated with the purely igneous rocks of the eastern parts of Cumberland and Westmoreland, which underlie the Coniston Limestone, and are of Caradoc age. This discovery has thus placed the green rocks of the Lake-country in the same position.

The Caradoc formation of the Lake-country was stated to embrace three divisions—namely, the Coniston Flags and Grits, the Coniston Limestone, and the Igneous rocks and ash-beds; and the following organic remains were enumerated as having been obtained

from the Coniston Flags and Grits, the uppermost division of the formation:—*Graptolithus Ludensis*, *Diplograpsus pristis*, *Phacops obtusicordatus*, *Orthis crispa*, *Cardiola interrupta*, *Orthoceras filosum*, *O. tenuistriatum*, and *O. subannulatum*.

4. "On the Lower Silurian Rocks of the Isle of Man." By Prof. R. Harkness, F.R.S., F.G.S., and H. Nicholson, Esq.

The older sedimentary deposits, which occupy the greater part of the island, have been regarded by previous observers as Lower Silurian. These slates were described by the authors as forming an anticlinal axis which traverses the island in a north-east and south-west direction, and to be conformably overlain at Douglas Head and Banks How on the south-eastern part of the island, by green ash-beds (slates and porphyries).

The only fossil of the slates is the *Palæochorda major* of the Skiddaw slates; and from the circumstance that the Lower Silurian rocks of the Isle of Man are in the exact line of strike of the Skiddaw slates of the Lake-country, the authors regarded these beds as corresponding with them; and the "green ash-beds" were considered to be the equivalents of the ash-beds and porphyries which succeed the Skiddaw slates.

LXXIX. Intelligence and Miscellaneous Articles.

ON THE LAW OF THE UNION OF SIMPLE SUBSTANCES, AND ON ATTRACTIONS AT SMALL DISTANCES. BY MM. ATHANASE AND PAUL DUPRÉ.

THE use of weighings in measuring work and molecular forces leads to a precision which had not been hitherto attained. It has become possible to commence the study of attractions at small distances; and there is reason to hope for an early and considerable progress in those branches of the physical sciences which are more directly connected with molecular mechanics.

When two bodies, terminated by plane parallel faces, approach till they are in contact, a work is effected proportional to the surface, and independent of the thickness, provided this exceeds the radius e of the sphere of sensible attraction. We have shown that the same number represents this work, the force of union, and the force of contraction of the superficial layer; the apparatus for determining it have been described (memoir addressed to the Academy in October 1865, and *Annales de Chimie et de Physique*, February, March, and April 1866). If reduced by calculation to the unit of surface and unit of specific gravity, the result obtained for each body becomes comparable with analogous numbers. The two plates which unite may moreover be of the same nature, or may have a different chemical composition; it is convenient to represent the force of union by a symbol f accompanied by a chemical indication of the bodies in question; thus $f_{\text{Hg}}^{\text{Hg}}$ represents the force of the union of mercury with itself after reduction to unity, and f_{H}^{O} that of hydrogen with oxygen. By the aid of this notation the force of union of a com-

pound body may be calculated as a function of the union of elements which it is often impossible to obtain in a direct manner. By equating the values found by experiment, an equation is obtained which contains in fact several unknown quantities; but other compounds containing the same simple substances in different proportions furnish new equations; and provided their number is sufficiently great, not only the numbers sought, but also very valuable verifications are obtained. This research, which will be communicated completely to the Academy when finished, has already given a most important law.

The forces of union of simple substances reduced to the unit of surface and of specific gravity are inversely proportional to their equivalents.

For hydrogen we have obtained 27 milligrammes per millimetre, and that by four methods:

- (1) By means of mercury.
- (2) By means of bromine.
- (3) By means of oil of turpentine and benzole.
- (4) By means of water, wood-spirit, and benzole.

But we only propose this figure as the result of a primary study of the fundamental number; it will probably undergo some corrections after the experiments have been renewed, and the products completely purified. The experiments to be made are long and delicate; great precautions must be taken to avoid serious errors.

The facts already confirmed refer to definite integrals; hence it is probable, though not quite certain, that

At the same very small distance, and with an equal specific gravity, two elements of volume of a simple substance exert on each other an attraction the value of which must be simply multiplied by the ratio of the equivalents if it is to be applied to another simple substance.

This law of attraction evidently reproduces the experimental law of the forces of union. It leads directly to several other laws which we shall examine experimentally, of which the following are the enunciations:—

In simple substances the attractions on contact, reduced to the unit of surface and of specific gravity, are inversely as the equivalents.

In simple substances the ratio of attraction on contact to the force of union is—

- (1) Independent of the specific gravity.
- (2) Independent of the chemical nature.
- (3) Independent of the molecular grouping.

For all simple substances taken in the fluid state, a constant product is obtained when the following four numbers, referred to the same temperature and pressure, are multiplied together:—

- (1) The chemical equivalent.
- (2) The coefficient of expansion for constant pressure.
- (3) The inverse of the coefficient of compressibility.
- (4) The inverse square of the density.

The attractions on contact of two different simple substances acting one upon another, and therefore of any two compounds, are obtained by calculations resembling those we have used for the forces

of union; the attractions of very close-lying parts enter into it evidently in great measure.

When a simple substance unites with another element, the force of union is not always attractive; and thus we have an explanation of facts hitherto difficult to understand—for instance, the heat produced by the decomposition of binoxide of nitrogen. The atoms of oxygen and those of nitrogen attract at distances greater than e with a force (universal gravity) which is only appreciable for large masses. At distances less than a certain quantity e' , very small as compared with e , they still attract; but in the interval ee' they repel one another, and the work of chemical union consists of one part negative and one part positive which is less. They do not combine directly, because they repel one another; but when by any means they are made to traverse the interval ee' , they may remain united. These latter views are partly hypothetical; hence we shall not fail to submit them to all the verifications of which they are capable. The principal difficulties arise from the circumstance that chemical products must be used which are very pure, and capable of furnishing the best determinations. With ternary compounds, if the choice is not good, the errors to be feared accumulate very rapidly in the calculation.

We have shown, by the known values of the forces of union and attraction on contact, that the general law of attraction would not be expressed by several terms obtained by multiplying whole powers of the inverse of the distance by constant coefficients. It would appear to be represented by the sum of three functions of the distance, of which the first (that is, the astronomical function) would predominate completely at great distances, and could be entirely neglected at small ones. The second, which might be called the physical function, would predominate in the interval ee' ; it would almost completely determine the force of union, and would be common to all simple substances, provided it were preceded by the inverse of the equivalent as a factor. The third, finally, (that is, the chemical function), would predominate in turn from zero to e' .—*Comptes Rendus*, April 2, 1866.

ON A NEW METHOD OF MEASURING THE LENGTHS OF LUMINOUS WAVES. BY PROF. STEFAN.

If light be allowed to fall on a column of quartz with polished faces parallel to the optic axis, each ray is resolved into the ordinary and the extraordinary ray, if the faces of entrance and of emergence are parallel. If both are again brought into a common direction of vibration, all those rays are extinguished the difference of whose path amounts to an uneven number of semi-wavelengths. If the spectrum of the light is formed, dark interference-bands appear, which are the more numerous and the finer the thicker the quartz. The difference of phase between two rays may be calculated from the thickness of the quartz, and from the quotients of refraction,—and with great accuracy, since only the differences, and not the absolute values of the latter are required. Twice the difference of phase divided by the wave-length is an uneven number for each dark band, and for each succeeding one towards violet is two units

greater. From the number of bands from one Fraunhofer's line to another, the wave-length of the latter may be calculated when that of the first is known.

To determine a wave-length directly, independent of another, the difference of phase for the place of the spectrum in question must be successively increased or diminished. Therefore a removal of the interference-bands sets in. From the number of the bands which have passed through the cross wires, and the change of the difference of phase thus produced, the wave-length can be calculated for the place fixed upon. The successive change of the difference of phase could be obtained by pushing over each other two quartz wedges. Such an apparatus was not at hand, and the following method was therefore used. The column of quartz was turned slowly out of its position at right angles to the incident rays; the angle of incidence, and therefore also the difference of phase between the ordinary and extraordinary ray successively increased, and the bands which simultaneously passed through the cross wire were counted. As the change in the difference of phase which occurs with the measured alteration of the incident angle can be calculated, these are the data needed for the absolute determination of the wave-length of the place fixed upon.

For the wave-lengths of Fraunhofer's lines B, C, D, E, F, G, H, the following numbers were found, in ten-millionths of a millimetre: 6873, 6578, 5893, 5271, 4869, 4291, 3959. These values agree very accurately with those deduced from the diffraction phenomena of fine gratings, and are therefore at the same time a proof of the accuracy of our theory of the diffraction of light.—*Berichte der Wiener Akademie*, April 26, 1866.

ON THE INFLUENCE OF INTERNAL FRICTION IN THE AIR ON THE MOTION OF SOUND. BY PROF. STEFAN.

The results of the analytical investigation are as follows. Friction increases the velocity of sound, and to a greater extent the higher the tone. Yet even for the highest tones this increase is very small, about 0.001 millim. in a second.

The amplitudes decrease in plane-progressive waves in geometrical progression. The exponent of this progression increases with the height of the tone, and, indeed, proportionally to the square of the number of vibrations. The diminution of amplitude is only perceptible in high tones. With a tone of 10,000 vibrations the amplitude is diminished by $\frac{1}{9}$ at a distance of 1000 metres; at 2000 metres by $\frac{1}{67}$; with a tone of 30,000 vibrations by $\frac{1}{6}$, even at 100 metres.

Standing vibrations are only possible if the length of the wave exceeds a certain value. Yet this is very small, equal to four times the mean way which, according to the new theory of gases, a molecule makes from one impact to the next.

In a standing wave also the amplitudes decrease with the time in geometrical progression, whose exponent is proportional to the square of the number of vibrations. The amplitudes of the tones of 1000, 10,000, and 30,000 vibrations sink to one-half before the lapse of 100 seconds, 1, and 0.1 second respectively.—*Berichte der Wiener Akademie*, April 16, 1866.

INDEX TO VOL. XXXI.

- ACETYLENE**, researches on, 456.
 Æther, on the resistance, elasticity, and weight of solar, 210.
 Alcohol, on the compounds of, with water, 137.
 Astronomical prolusions, 52, 287.
 Athanase (M.) on the law of union of simple substances, and on attractions at small distances, 548.
 Atkinson (Dr. E.), chemical notices by, 137, 306, 451.
 Atmosphere, on the diminution of direct solar heat in the upper regions of the, 104, 261.
 Barytine, on some crystalline forms of, 179.
 Baudrimont (M.) on white phosphorus, 144.
 Bauer (M.) on benylene, 455.
 Bauerman (H.) on the copper-mines of the State of Michigan, 482.
 Beketoff (M.) on the displacement of some elements by others, 306.
 Benylene, on the preparation and properties of, 455.
 Benzenic acid, on the preparation and properties of, 453.
 Berthelot (M.) on acetylene, 456.
 Birds, on the functions of the air-cells and the mechanism of respiration in, 230.
 Books, new :—Walton's Mathematical Writings of D. F. Gregory, 76 ; Pratt's Treatise on Attractions, 144.
 Boron, on the forms of graphitoidal, 397.
 Brewster (Sir D.) on the bands formed by the superposition of paragenic spectra, 22, 98.
 Briot (M.) on the measurement of small forces by means of the pendulum, 160.
 Broughton (J.) on some properties of soap-bubbles, 228.
 Browne (G. F.) on ice-caves, 82.
 Calculus of variations, on the solution of a problem in, 218, 425.
 Calorescence, researches on, 386, 435.
 Cambridge Philosophical Society, proceedings of the, 78, 230, 315.
 Carius (M.) on a new saccharine substance from benzole, 452.
 Caron (M.) on the occurrence of niobium and tantalum in a tin ore, 142.
 Cartesian ovals, on the focal theory of, 52, 287, 380.
 Cayley (Prof.) on a new theorem on the equilibrium of four forces acting on a solid body, 78.
 Cazin (A.) on the expansion of saturated vapours, 163.
 Challis (Prof.) on hydrodynamics, 33 ; on the solution of a problem in the calculus of variations, 218 ; on the motion of a small sphere acted upon by the undulations of an elastic fluid, 343 ; on the fundamental ideas of matter and force in theoretical physics, 459.
 Chances, on some problems in, 170.
 Chapman (Prof. E. J.) on some minerals from Lake Superior, 176.
 Chemical notices, 137, 306, 451.
 Clarke (Capt. A. R.) on the figure of the earth, 193.
 Clausius (Prof.) on the determination of the disgregation of a body, and on the true capacity for heat, 28.
 Coal, on the conditions of the deposition of, 158.
 Colour-disease, on the doctrine of, 85.
 Cooke (J. P.) on the construction of

- a spectroscope with a number of prisms, 110; on the heat of friction, 241; on the aqueous lines of the solar spectrum, 337.
- Copper-mines of Michigan, remarks on the, 482.
- Croll (J.) on the excentricity of the earth's orbit, 26; on the physical cause of the submergence and emergence of the land during the glacial epoch, 301.
- Dawson (Dr. J. W.) on the conditions of the deposition of coal, 158.
- De la Rue (W.) on the decrease of actinic effect near the circumference of the sun, 243.
- De Wilde (M.) on acetylene, 456.
- Diacon (M.) on the influence of the electro-negative elements on the spectra of the metals, 483.
- Drosier (Dr.) on the functions of the air-cells, and the mechanism of respiration in birds, 230.
- Dupré (P.) on the law of the union of simple substances, and on attractions at small distances, 548.
- Earth, on the change of excentricity of the orbit of the, as a cause of change of climate, 26, 374; on the axial rotation of the, 210, 323; on the retardation of the velocity of rotation of the, 322; on the fluid theory of the, 430; on the observations and calculations required to find the tidal retardation of the rotation of the, 533; on a possible geological cause of changes in the position of the axis of the crust of the, 537.
- Earthquakes, observations on, 45.
- Edlund (E.) on the relation between the heat disengaged by induction-currents and the mechanical force employed to produce it, 253.
- Edmonds (R.) on earthquakes and extraordinary agitations of the sea, 45.
- Edmonds (T. R.) on the law of human mortality expressed by a new formula, 1.
- Electric spark, on the heat of the, 427.
- Electrical resistance, on the unit of, 325, 376.
- Electrodes, on the explosive distance of the direct induced current between similar, 107.
- Elements, on the refractive equivalent of the, 483.
- Equation, on the separation of the roots of an algebraical, 214.
- Euler's theorem, on an instantaneous proof of, 52.
- Evans (J.) on geological changes in the position of the axis of the earth's crust, 537.
- Everett (Dr. J. D.) on the flexural and torsional rigidity of a glass rod, 476.
- Farmer (M. G.) on the mechanical equivalent of light, 403.
- Feldmann (M.) on laserpitine, 451.
- Fick (Prof.) on the retardation of the earth's velocity of rotation, 322; on the origin of muscular power, 485.
- Force and matter, on the fundamental ideas of, 459.
- Forces, on the composition of, 245, 404.
- Frankland (Prof. E.) on St. Elmo's fire, 321.
- Gases, studies on, 124, 181; on the electrical conductivity of, under feeble pressures, 319.
- Geological Society, proceedings of the, 155, 237, 318, 399, 477, 545.
- Gill (J.) on regelation, 119.
- Glacial epoch, on the level of the sea during the, 172, 301, 372, 532.
- Glass, on the coloration of, by selenium, 84; on the rigidity of, 476.
- Glennie (J. S. S.) on the axial rotation of the earth, 323.
- Guthrie (Prof. F.) on the axial rotation of the earth, and the resistance, elasticity, and weight of solar æther, 210.
- Haughton (Rev. S.) on the change of excentricity of the earth's orbit as a cause of change of climate, 374.
- Heat, on the true capacity for, 28; on diminution of direct solar, in the upper regions of the atmosphere, 104, 261; on the mechanical equivalent of, 135; of friction, on the, 241; on the relation between the, disengaged by induction-currents, and the mechanical force employed to produce it, 253.
- Heath (D. D.) on secular local changes in the sea-level, 201, 323.

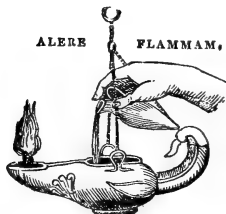
- Heddle (Dr.) on the occurrence of Wulfenite in Scotland, 253.
- Hittorf (M.) on the various modifications of phosphorus, 311.
- Hoffmann (Prof.) on the preparation of solutions of peroxide of hydrogen, 143.
- How (Prof.) on the mineralogy of Nova Scotia, 165.
- Huggins (W.) on the spectrum of comet 1, 1866, 233; on the spectra of some of the fixed stars, 405, 515; on the spectra of some of the nebulae, 475, 523.
- Human mortality, on the law of, 1.
- Hydrodynamics, researches in, 33.
- Hydrogen, on the preparation of solutions of peroxide of, 143.
- Ice-caves, notes on some, 82.
- Induction-currents, on the heat disengaged by, 253.
- Jamin (M.) on the measurement of small forces by means of the pendulum, 160.
- Jukes (J. B.) on the carboniferous slate of North Devon and South Ireland, 477.
- Kekulé (Prof.) on the constitution of the aromatic compounds, 456.
- Land, on the physical cause of the submergence and emergence of the, during the glacial epoch, 301.
- Lambert's theorem, on an instantaneous proof of, 52.
- Laserpitine, researches on, 451.
- Lead, on the occurrence of native, 176.
- Light, on the mechanical equivalent of, 403; on a new method of measuring the lengths of waves of, 550.
- Loewy (Mr.) on the decrease of the actinic effect near the circumference of the sun, 243.
- Magnetic bar, on the changes which stretching and the passage of a voltaic current produce in a, 239.
- dip, on the secular change of, 235.
- Magnetism, terrestrial, observations on, 265.
- Manganite, analyses of, 166.
- Marcasite, on the occurrence of, 178.
- Matter and force, on the fundamental ideas of, 459.
- Matthiessen (Prof. A.) on the expansion of water and mercury, 149; on the unit of electrical resistance, 376.
- Mendelejeff (M.) on the compounds of alcohol with water, 137.
- Mercury, on the expansion of, 149; on the specific gravity of, 316.
- Metals, on the influence of the electro-negative elements on the spectra of the, 483.
- Miller (Prof. W. A.) on the spectra of some of the fixed stars, 405, 515.
- Miller (Prof. W. H.) on the crystalline forms of some compounds of thallium, 153; on the forms of graphitoidal silicon and graphitoidal boron, 397.
- Mineralogy of Nova Scotia, contributions to the, 165.
- Minerals from Lake Superior, on some, 176.
- Moore (J. C.) on glacial submergence, 372.
- Morren (A.) on the electrical conductivity of gases, 319.
- Muscular power, on the origin of, 485.
- Naphthaline, on a new tetratomic alcohol from, 454.
- Nebulae, on the spectra of some of the, 475, 523.
- Neuhoff (M.) on naphthendichlorhydride, 454.
- Neumayer (G.) on aqueous vapour and terrestrial radiation, 510.
- Newton's rule, on the demonstration of, 369.
- Niobium, on the occurrence of, in tin ore, 142.
- Norton (Prof. W. A.) on molecular physics, 265.
- Oils, mineral, on a method of testing, 143.
- Orbit, on the periodical changes of, 287.
- Ozone, on the density of, 82.
- Paalzow (Dr. A.) on the heat of the electric spark, 427.
- Paragenesis spectra, on the bands formed by the superposition of, 22, 98.
- Pelouze (J.) on the coloration of glass by selenium, 84.
- Pendulum, on the measurement of small forces by means of the, 160.
- Pentole, on the composition and properties of, 454.

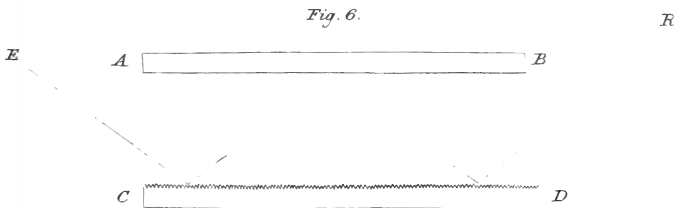
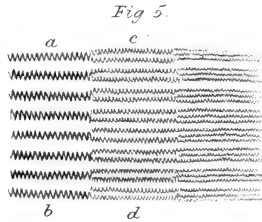
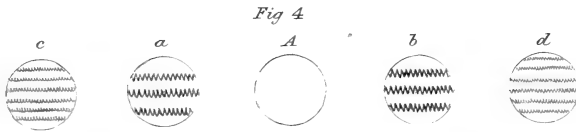
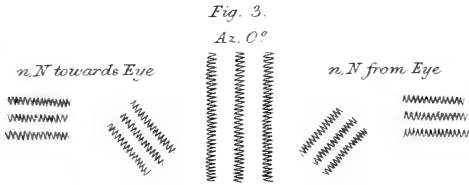
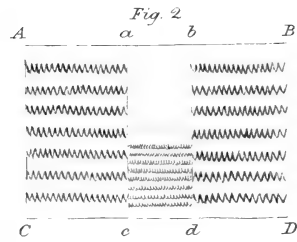
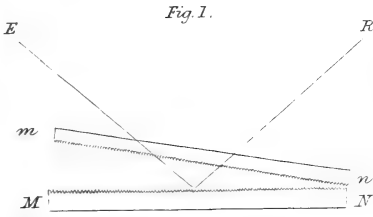
- Phenose, on the preparation and properties of, 452.
- Phosphorus, on white, 144; researches on, 311.
- Physics, on molecular, 265.
- Planetary motion, on motion in a circle and its relation to, 52.
- Polynomials, on a general property of derived, 369.
- Pratt (Archdeacon) on the level of the sea during the glacial epoch, 172, 532; on the fluid theory of the earth, 193, 430.
- Pyrolusite, analyses of, 167.
- Radiation, on terrestrial, 510.
- Rankine (W. J. M.) on saturated vapours, 197, 199.
- Regelation, observations on, 119.
- Reusch (E.) on a gas-burner for sounding large tubes, 401.
- Rose (Dr. E.) on the doctrine of colour-disease, 85.
- Royal Society, proceedings of the, 149, 233, 316, 397, 475, 537.
- St. Elmo's fire, note on, 321.
- Salleron (M.) on a method of testing mineral oils, 143.
- Saturation, on the doctrine of uniform and constant, 283.
- Schrauf (A.) on the determination of the refractive equivalent of the elements, 483.
- Schroeder van der Kolk (Dr. H. W.) on gases, 124, 181.
- Schwendler (L.) on the galvanometer resistance to be employed in testing with Wheatstone's diagram, 364.
- Sea, on extraordinary agitations of the, 45; on the level of the, during the glacial epoch, 172, 201, 305, 323, 532.
- Secchi (Father) on the relation between the variation of sun-spots and that of the amplitude of magnetic oscillation, 324.
- Sedgwick (Prof.) on the geology of the valley of Dent, with some account of a destructive avalanche which fell in 1752, 79.
- Selenium, on the coloration of glass by, 84.
- Siemens (W.) on the unit of electrical resistance, 325, 376.
- Silicon, on the forms of graphitoidal, 397.
- Soap-bubbles, on some properties of, 228.
- Sodium-ethyle, on the action of carbonic oxide on, 505.
- Solar spectrum, on the aqueous lines of the, 503.
- Soret (M.) on the density of ozone, 82.
- Sound, on the influence of internal friction in the air on the motion of, 551.
- Spectra of the metals, on the influence of the electro-negative elements upon the, 483; of the fixed stars, on the, 405, 515; of the nebulae, on the, 475, 523.
- Spectroscope, on the construction of a, 110.
- Spectrum, on the aqueous lines of the solar, 337.
- Sphere, on the motion of a small, acted upon by the undulations of an elastic fluid, 343.
- Stars, on the spectra of some of the fixed, 405, 515.
- Stefan (Prof.) on a new method of measuring the lengths of luminous waves, 550; on the influence of internal friction in the air on the motion of sound, 551.
- Stevelly (Prof. J.) on the composition of forces, 245, 404.
- Stewart (B.) on the secular change of magnetic dip, 235; on the decrease of the actinic effect near the circumference of the sun, 243; on the specific gravity of mercury, 316; on the aqueous lines of the solar spectrum, 503.
- Sun, on the decrease of actinic effect near the circumference of the, 243.
- Sun-spots, on the relation between the variation of, and that of the amplitude of magnetic oscillation, 324.
- Sylvester (Prof. J. J.), astronomical prolusions by, 52; on the separation of the roots of an algebraical equation, and on a new theorem, 214; on periodical changes of orbit, with a new theory of the analogues to the Cartesian ovals in space, 287, 380.
- Thallium, on the crystalline forms of some compounds of, 153.
- Thermometer, on the black-bulb, 191.

- Thomson (Prof. W.) on secular local changes in the sea-level, 305; on the tidal retardation of the earth's rotation, 533.
- Tidal retardation of the earth's rotation, on the, 533.
- Todhunter (I.) on a problem in the calculus of variations, 425.
- Tyndall (Prof.) on the black-bulb thermometer, 191; on calorescence, 386, 435.
- Urbain (M.) on a method of testing mineral oils, 143.
- Vapour, aqueous, on the absorption of heat by, 510.
- Vapours, on the expansion of saturated, 163, 197, 199.
- Villari (M.) on the changes which stretching and the passage of a voltaic current produce in a magnetic bar, 239.
- Voltaic conduction, note of an experiment on, 83.
- Wanklyn (Prof. J. A.) on the doctrine of uniform and constant saturation, 283; on the action of carbonic oxide on sodium-ethyle, 505.
- Water, on the expansion of, 149.
- Waterston (J. J.) on voltaic conduction, 83.
- Wartmann (Prof. E.) on the explosive distance of the direct induced current between electrodes of the same kind, 107.
- Wheatstone's diagram, on the galvanometer-resistance to be employed in testing with, 364.
- Wilson (J. M.) on the diminution of direct solar heat in the upper regions of the atmosphere, 104, 261; on some problems in chances, 170.
- Wislicenus (Prof. J.) on the origin of muscular power, 485.
- Wulfenite, on the occurrence of, in Kirkeudbrightshire, 253.
- Young (Prof. J. R.) on the demonstration of Newton's rule, and on a general property of derived polynomials, 369.

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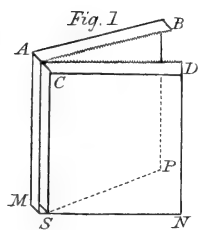


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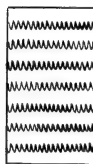


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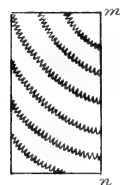


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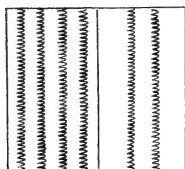


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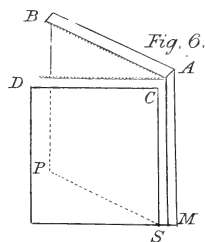
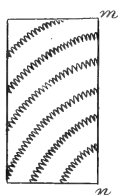


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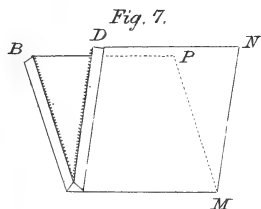
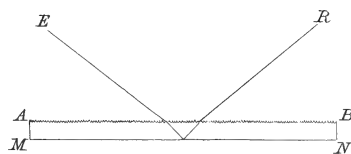


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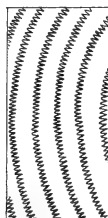
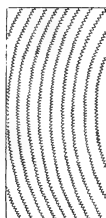


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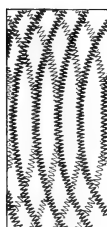
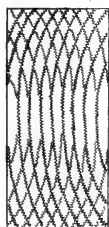


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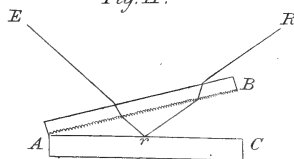




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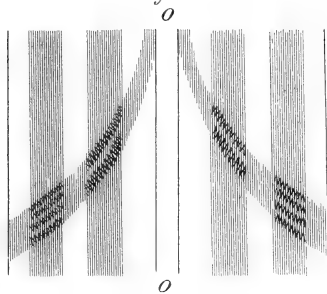


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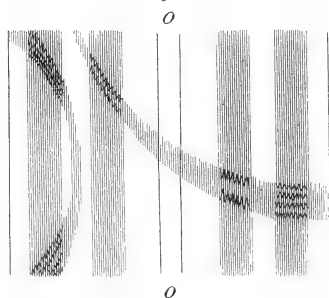


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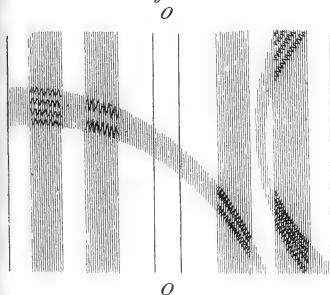


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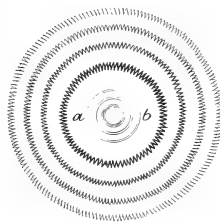


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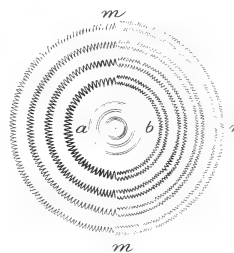


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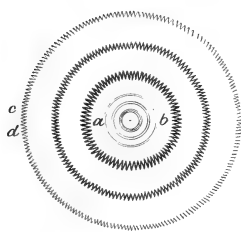


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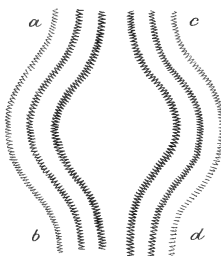


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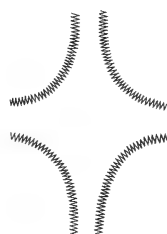


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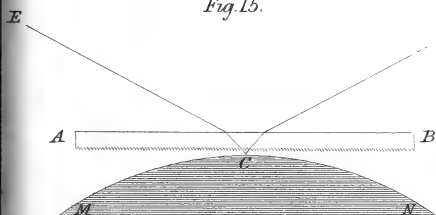
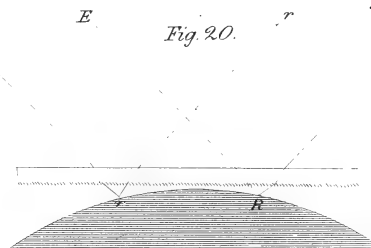


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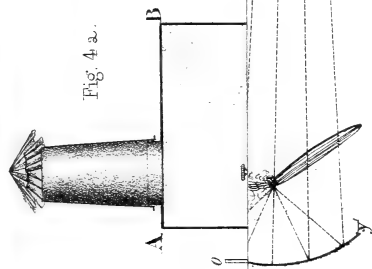


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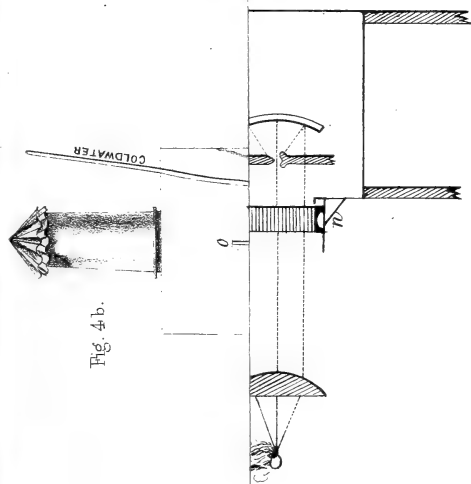


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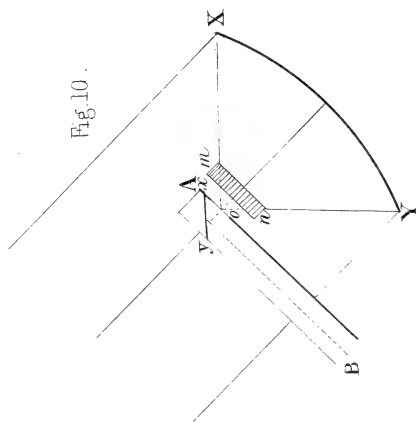


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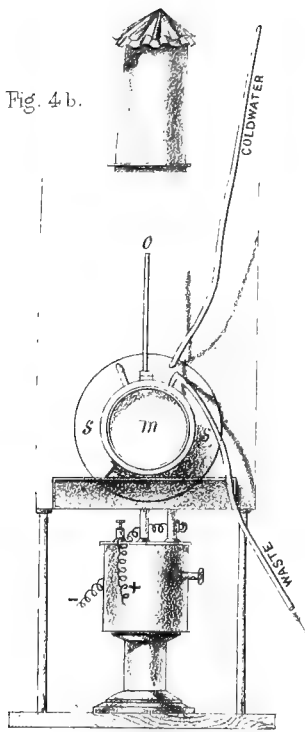


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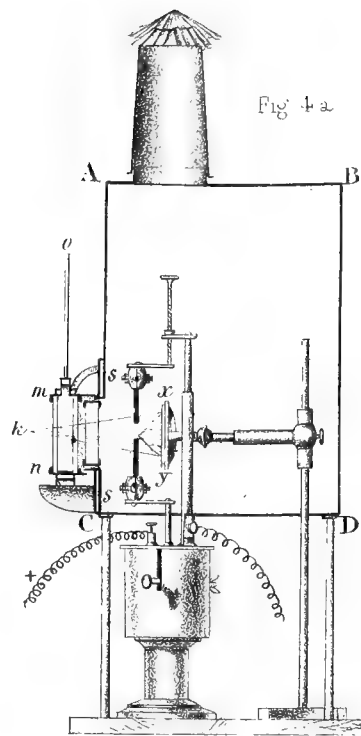


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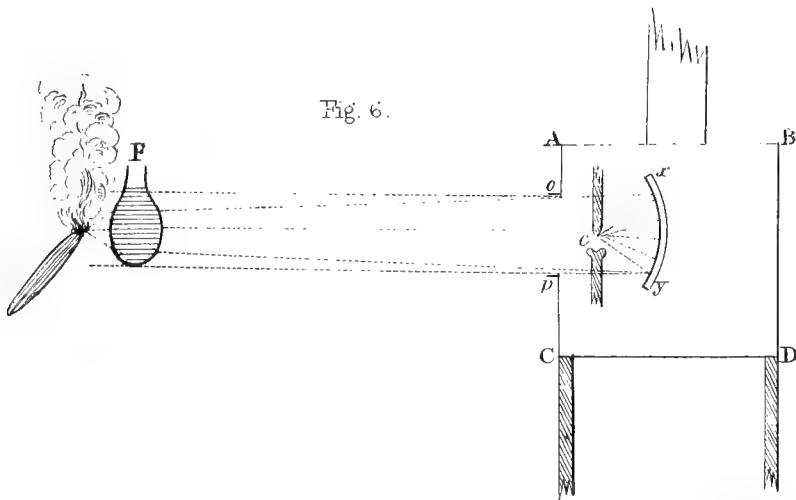


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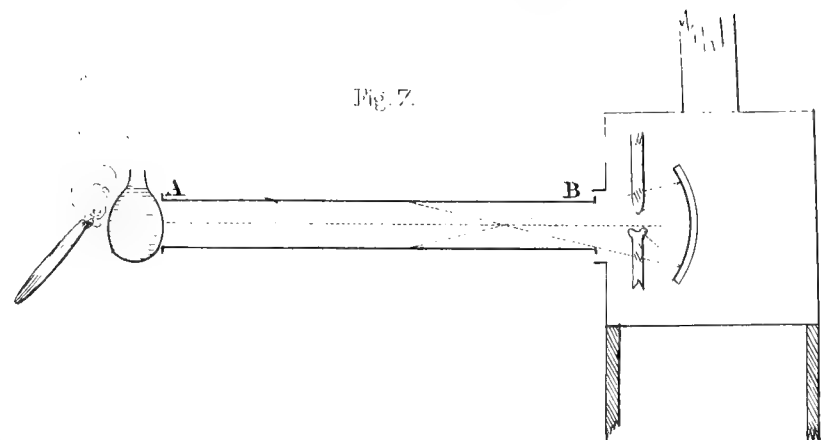


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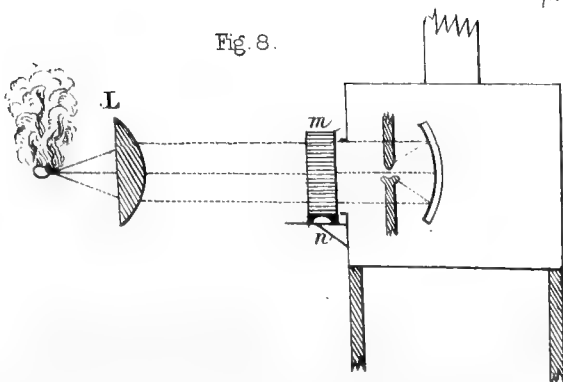


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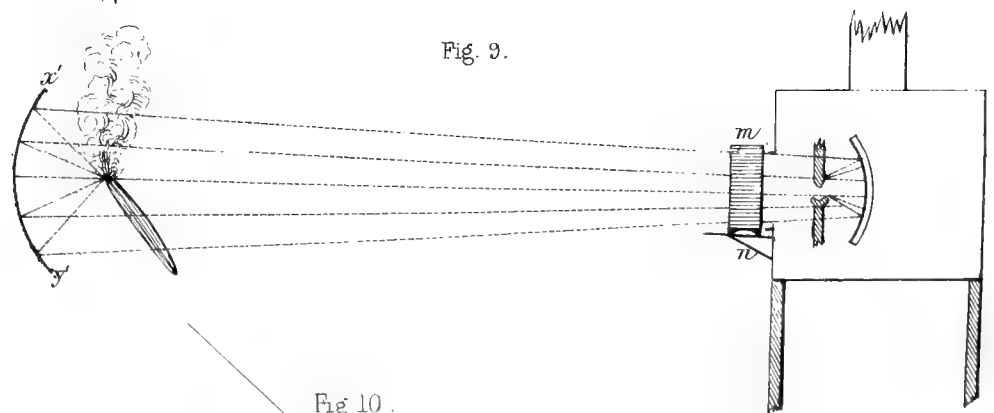
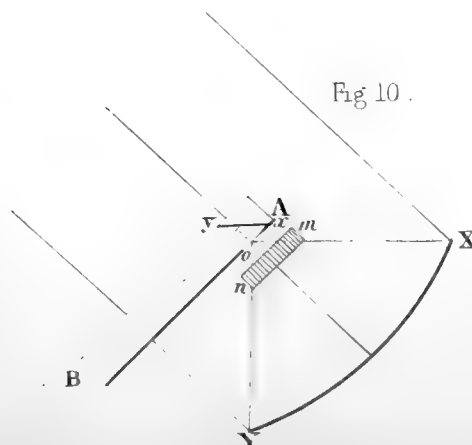
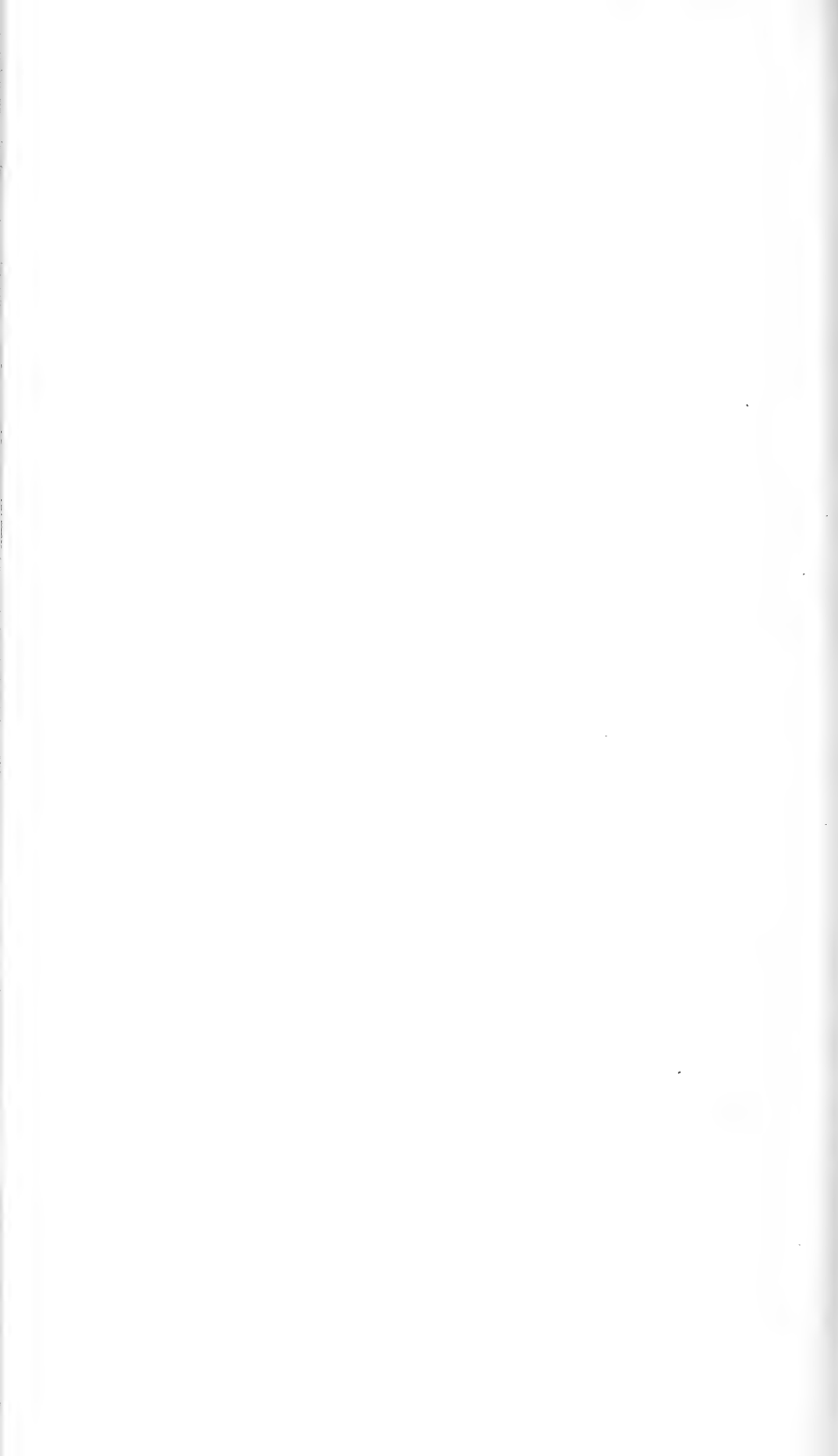


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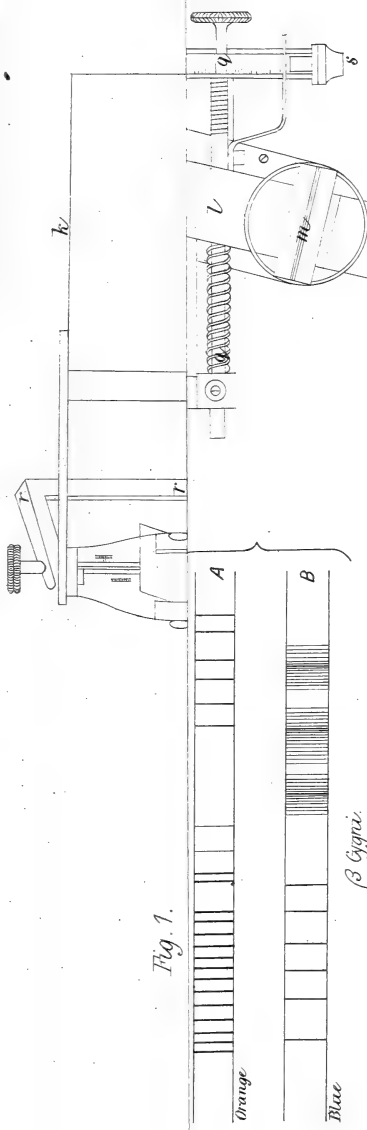


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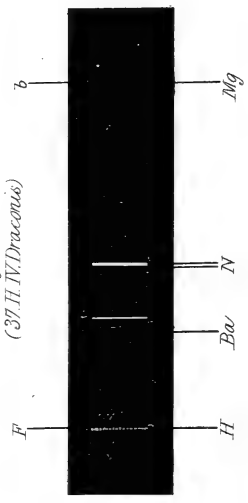


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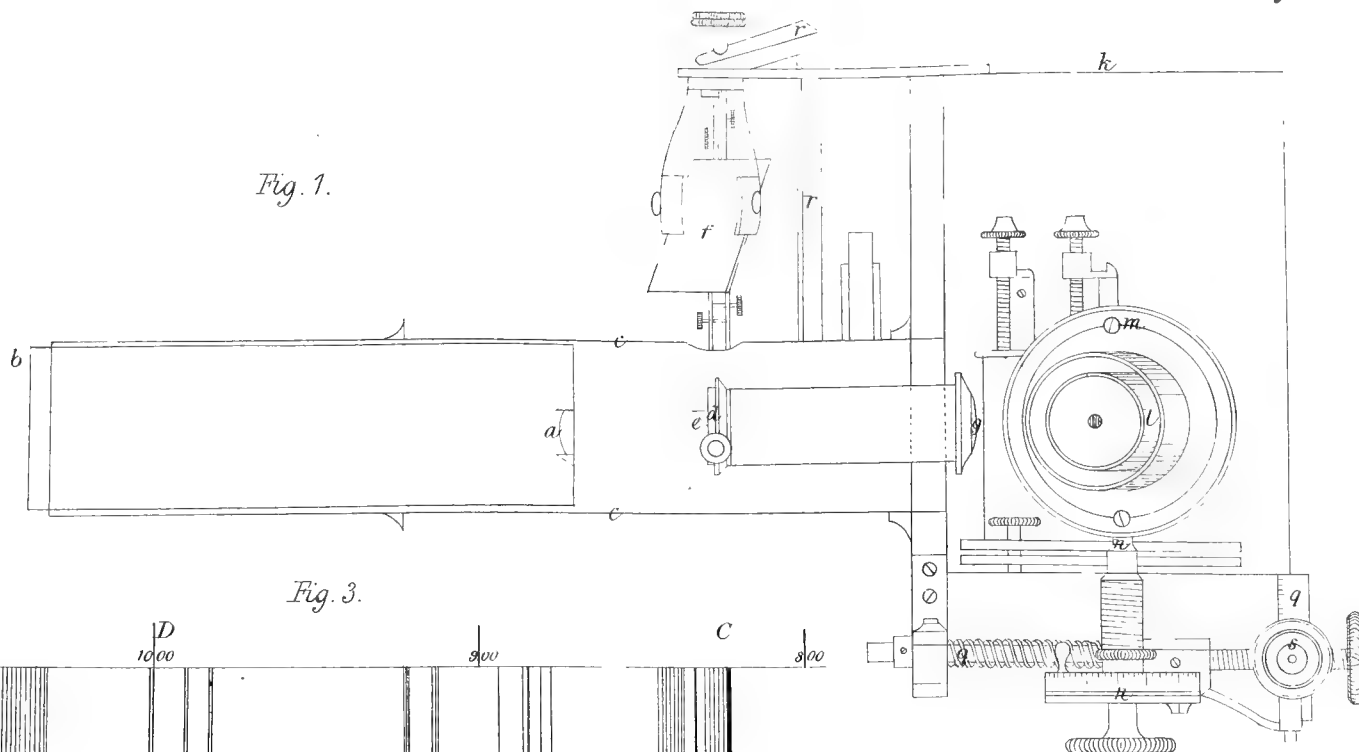


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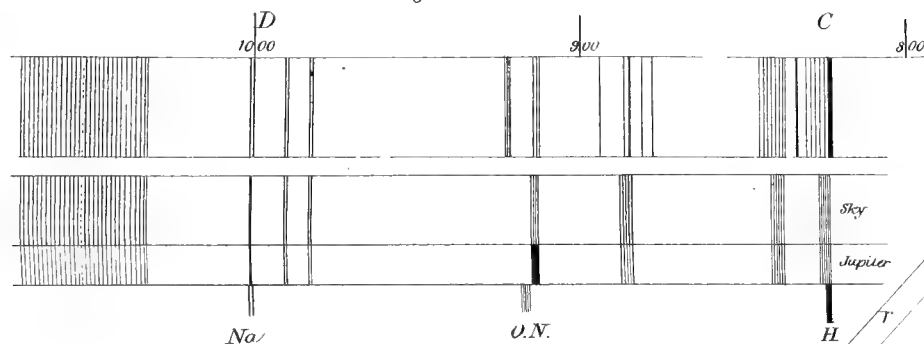


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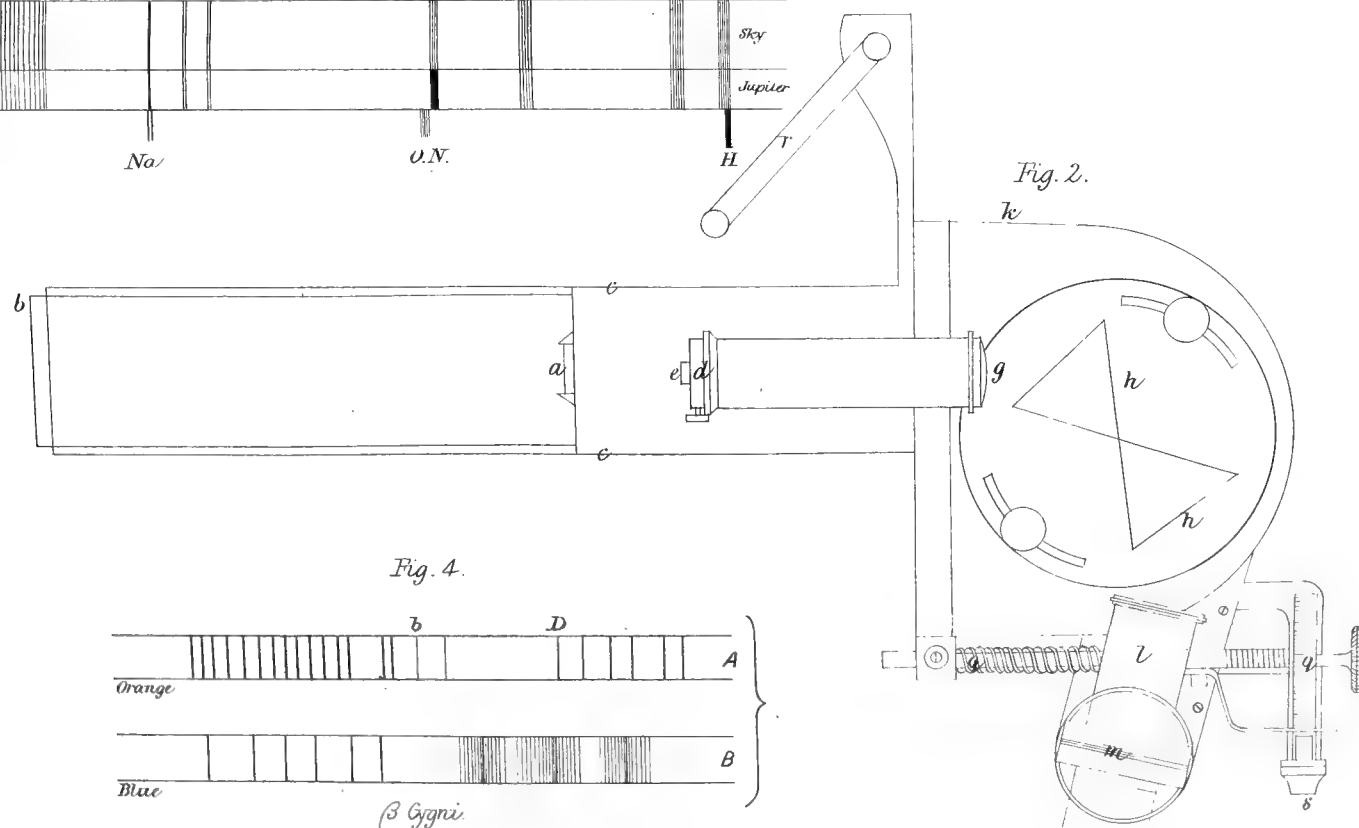


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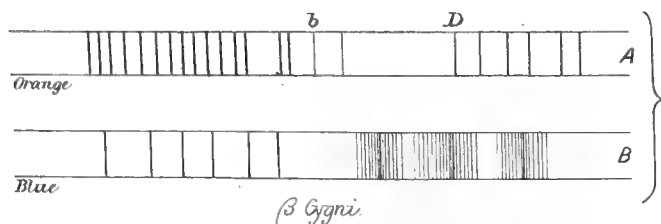
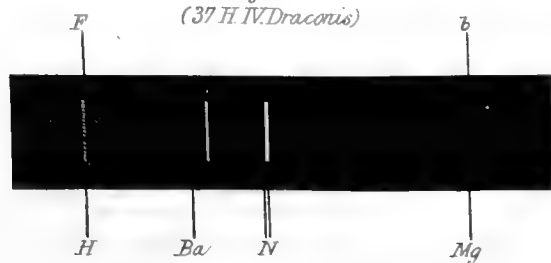
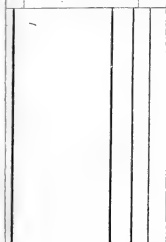


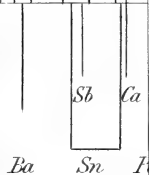
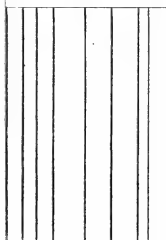
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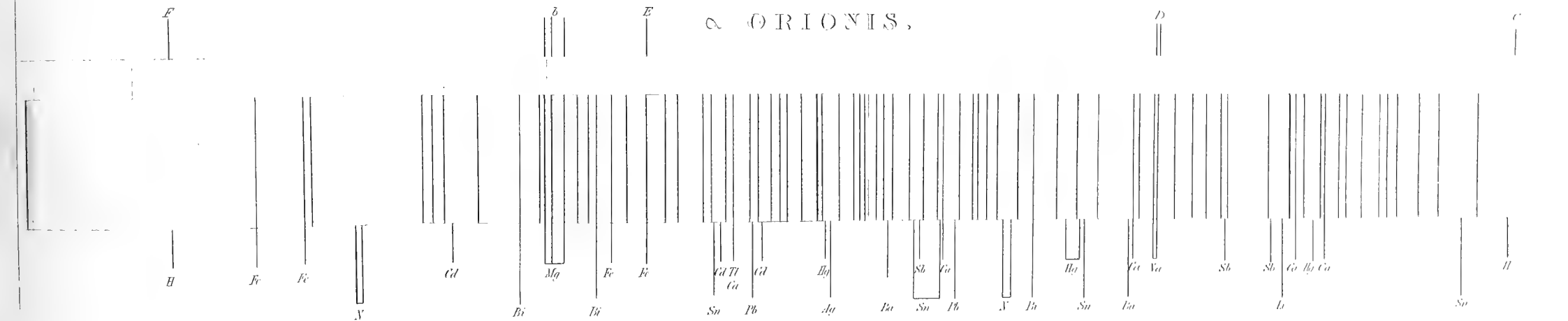


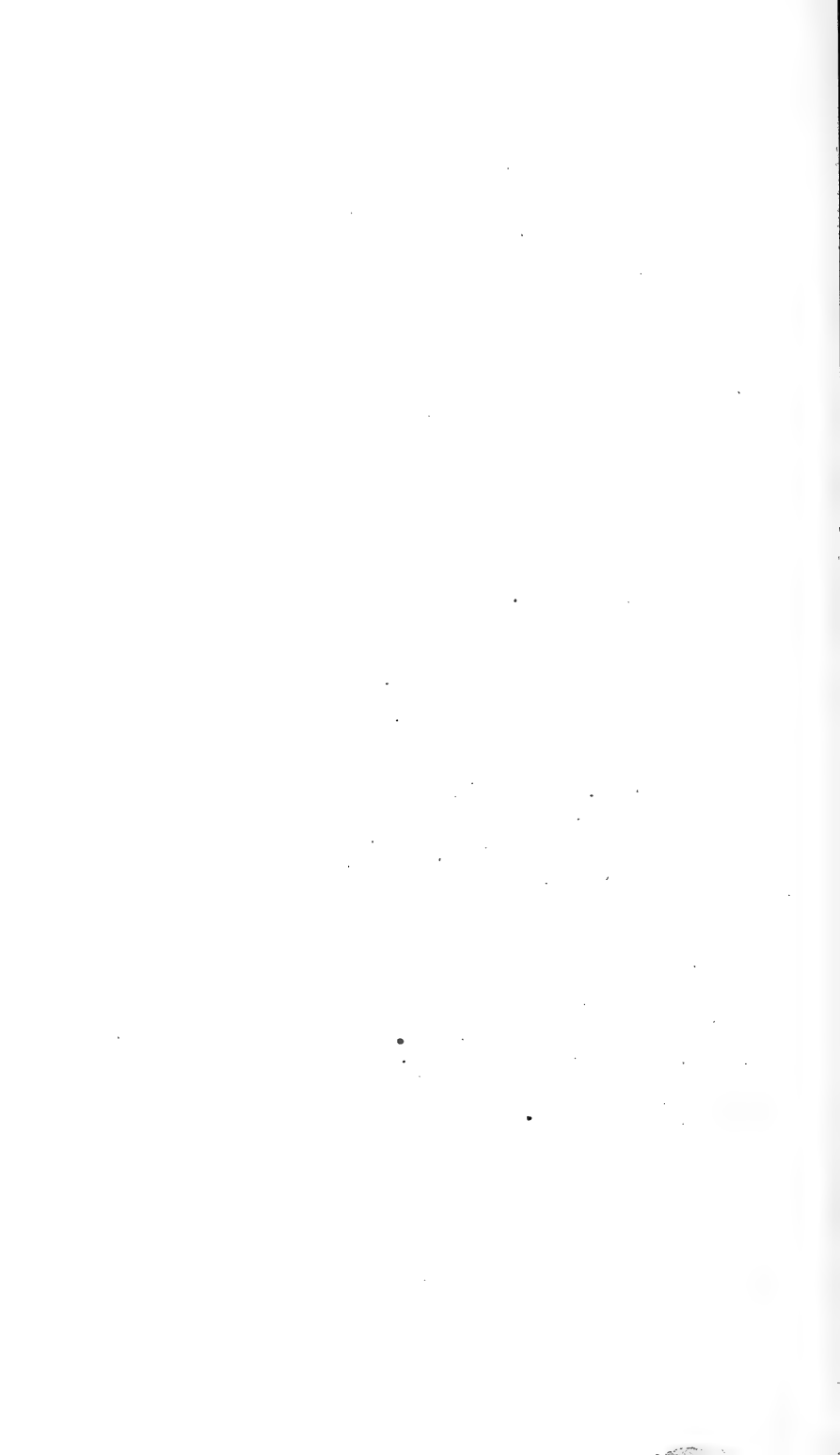
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N° 206.—JANUARY 1866.

WITH A PLATE,

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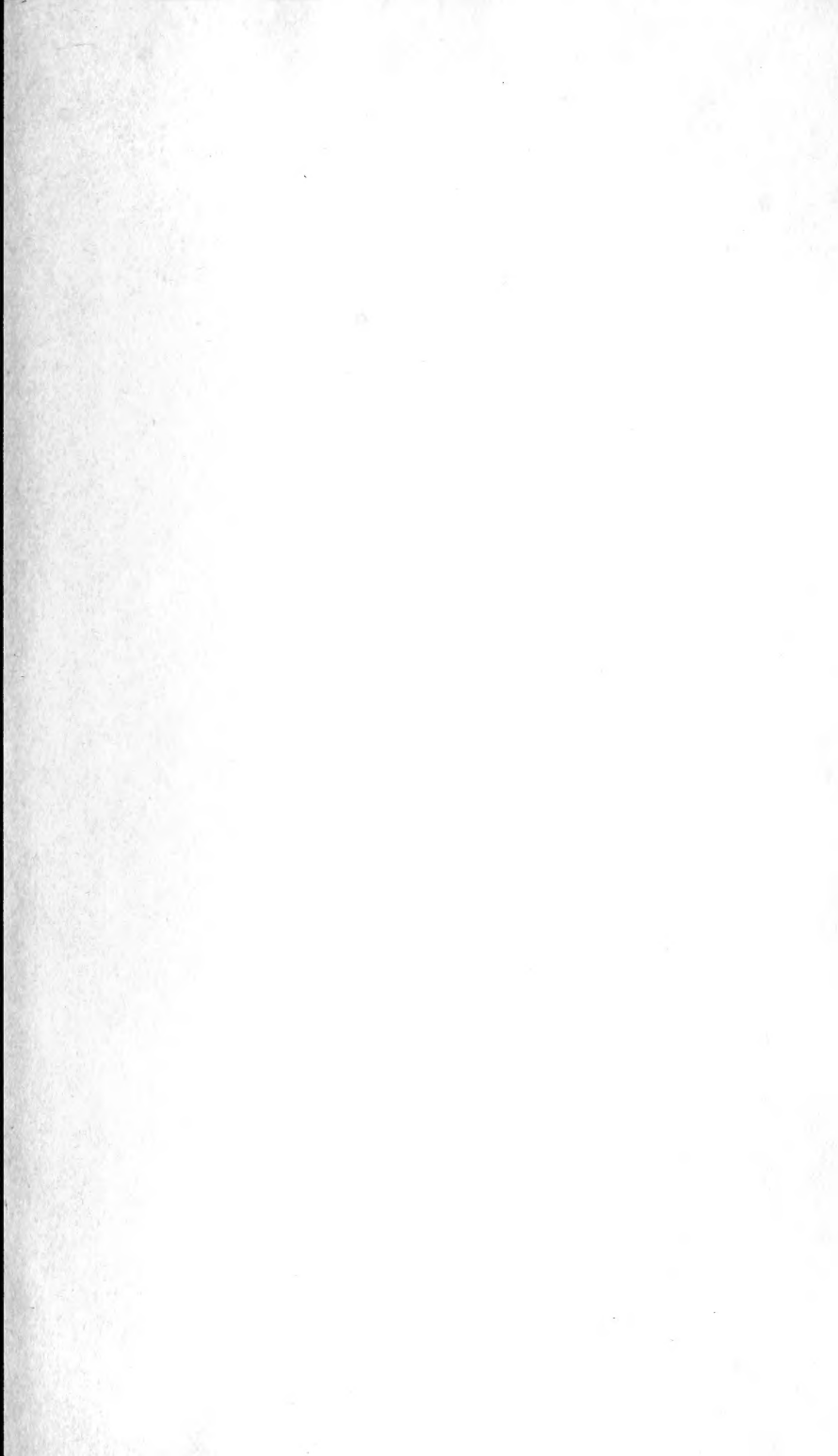
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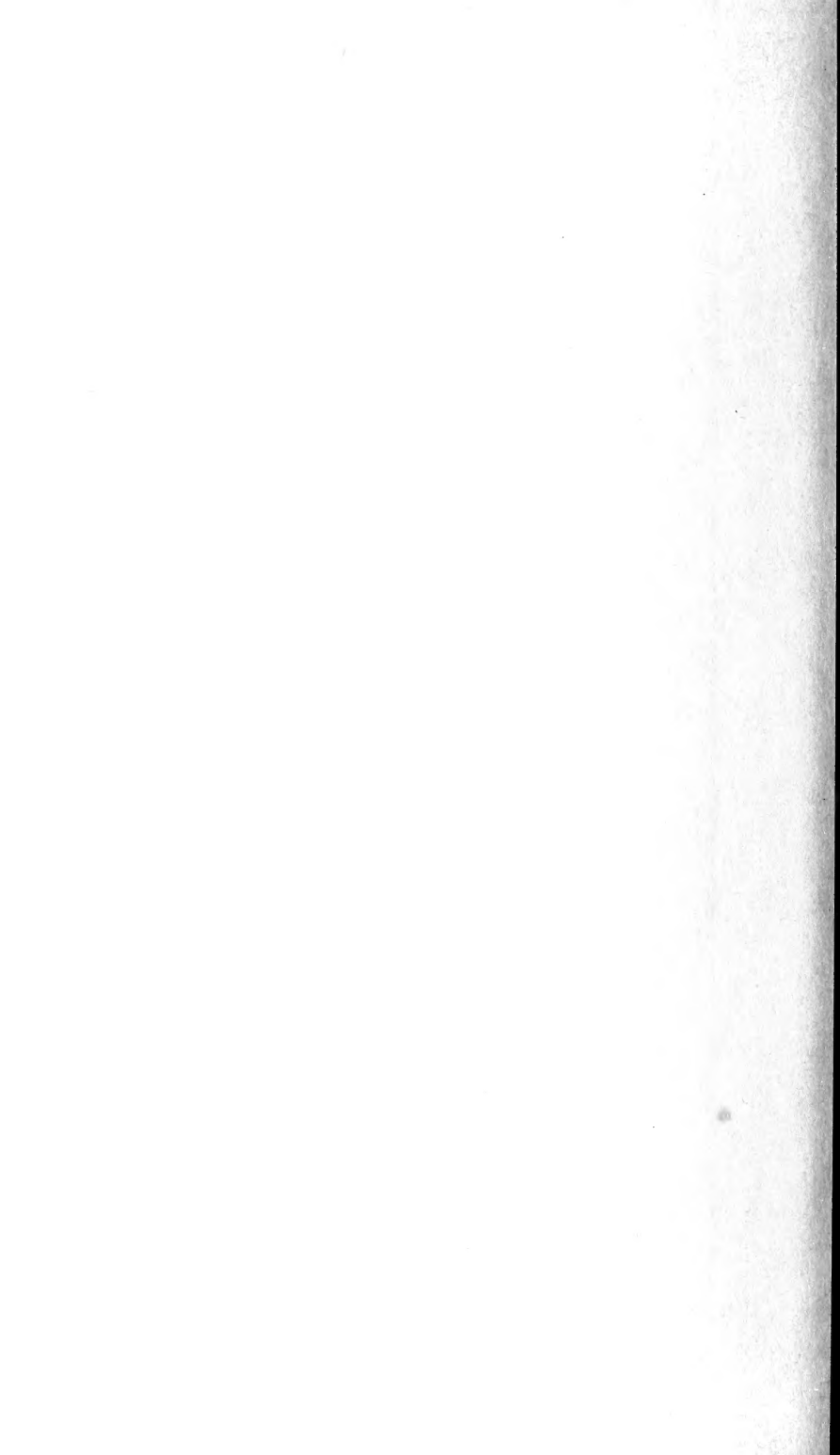
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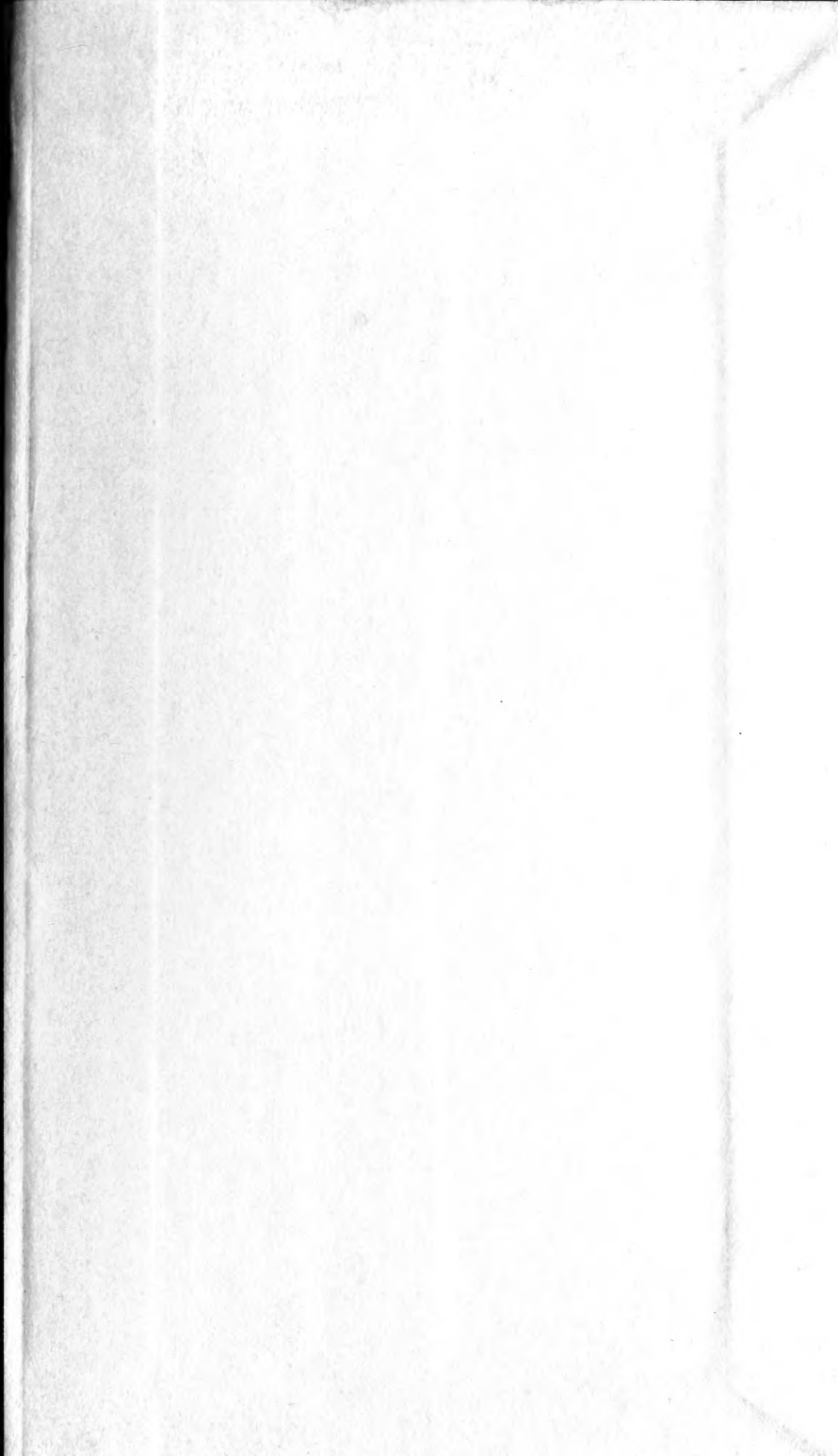
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